The Journal of the American Medical Association

VOL. XXII. CHICAGO, FEBRUARY 24, 1894. No. 8.

ORIGINAL ARTICLES.

MEDICAL EXPERT TESTIMONY.

BY HENRY M. LYMAN, A.M., M.D.

CHICAGO, ILL.

At a recent meeting of the Practitioners' Club in this city, an hour was devoted to a discussion of the subject of expert medical testimony. There appeared to be a feeling that the method of eliciting such testimony now in vogue in the law courts of the country is imperfect, and unjust to both the accuser and the accused. An ideal picture was presented for admiration, in which judges, jurors and lawyers were depicted at the feet of an impartial and omniscient medical expert (appointed by some rather nebulous power dimly visible in the back-ground), calmly engaged in registering his decrees in all cases of medical jurisprudence. Undoubtedly, this would be the best way in an ideal society, where medical experts were omniscient and every one else was eager for the truth, the whole truth, and nothing but the truth. Unfortunately, however, we do not live under such a dispensation of Providence. In an Utopia such as we delight to contemplate, crimes would be unknown, and medical experts would be unneeded. We must look to a lower sphere—such an one as we now occupy—for the opportunity to deploy medical expert testimony. In our human law courts one soon discovers that the truth, the whole truth, and nothing but the truth is by no means the object of research. On the contrary, the entire effort is directed to the winning of the suffrages of a jury in favor of one of two hypotheses which are laid before it. The success of a given hypothesis is the main thing in view; the conformity of that hypothesis with truth is quite a secondary consideration in the minds of the contestants. For this reason, in our American courts of law the power of the judge has been largely taken from him, so that he can not, like an European judge, intervene to any considerable extent, in the interest of truth and justice. For this reason, the majority of lawyers would resent the appointment of an impartial expert who should act as an assessor with the judge, and should instruct the jury in their course of action. So long as our judges are active politicians, selected by politicians for their availability rather than for their ability, their appointment of experts would always be liable to challenge for want of impartiality. The experiment of placing in their hands the work of appointing certain officers has been tried in past years, without giving to the public any better results than those obtained by the ordinary methods of popular election.

For these reasons it seems to me very improbable that, under existing conditions, any improvement upon the present methods of selecting medical experts can be expected. At present, each side in a court of law selects the experts in whom it feels the most confidence. Trivial cases conducted by inferior lawyers bring forward so-called experts who belong to the same level; while important cases involving great interests seldom fail to secure the services of the leaders in the professions of law and medicine. So long as the contestants are satisfied it is hardly expedient for us to complain. If the practice of law should ever rise to the level of the pursuit of the exact sciences in its devotion to truth, and its impartial search for facts, it will then be time to change the present method of selecting medical expert witnesses.

The outcry that is made against the present method of employing expert testimony has its origin, not so much with the heedless public, or with the self-satisfied lawyers, as with certain medical gentlemen who have sometimes found themselves at variance with the opinions of other physicians whose qualifications they are disposed to underrate. Such differences of opinion are seldom concerned with demonstrations of fact, but with matters of inference. For example, there is seldom any difference regarding the acceptation of proven symptoms as either genuine or fictitious. It is not often difficult to decide from an array of facts, whether a given individual is or is not of perfectly sound mind; but it is often very difficult to decide how far such an individual may or may not be responsible for his acts. Here lies the difficulty of agreement. This difficulty is increased by the fact—too often overlooked—that responsibility is a variable quantity. No one can listen to or read the autobiographic narratives of the insane without being impressed with this fact. The growth and progress of irresponsibility, the constant see-saw of conflict between opposing motives, and the final submergence of prudential considerations are the most interesting features of such histories. It is not among the chronic and violently insane patients who populate our asylums that these things are to be studied. It is among the slightly defective examples of idiocy and paranoia who still enjoy their freedom, and are yet unknown to the asylum physician, that these incipient degrees of irresponsibility must be investigated.

The difficulty of agreement among experts in the matter of insanity is farther increased by the fact that law and medicine do not approach the subject by the same paths. The physician simply endeavors to find out whether or no there is deviation from the normal standard of development or of health. If there be such deviation, he is naturally impressed by that fact, and feels that it should have weight in determining the degree of responsibility of the patient. Here lies the difficulty, and here begins the divergence of opinions. Uniformity of opinion regarding a variable quantity is out of the question; nor should its absence be urged as an argument against expert testimony, any more than the fact that

misdemeanors are lightly punished by one judge, but are severely visited by another, should be thrown in the face of an honorable judiciary. Furthermore, while the physician occupies himself with the scientific aspects of the case, the lawyer cares little for the physical condition of the malefactor—he is chiefly concerned with the results of his action and with the question of his responsibility. If, now, the lawyer were zealous only for the determination of the degree of responsibility, he would not often find himself in conflict with the physician. On the contrary, however, he assumes at the outset for the prisoner a certain arbitrarily asserted degree of responsibility; and during the course of the trial he seeks to include all testimony that is in harmony with that proposition, and to exclude everything that tends to controvert his assumption. This method of proceeding is favored by the whole process of the courts, in which the judge possesses little power beyond that of a moderator. The introduction of hypothetic questions also renders it possible to extract a favorable opinion from almost any witness who speaks about matters of inference. Additional doubt may be also created by the discord between legal and medical definitions of insanity; for it is a fact that a man may be legally sane though medically insane.

In all cases involving a question of responsibility, the medical witness must seek to avoid all show of active partisanship. He is free to demonstrate every defect, degeneration or disease that may tend to impair such responsibility, but he must not assume that all responsibility is necessarily abolished by the existence of such defect, degeneration or disease. Nor is it for him to determine, under our laws, the degree of extenuation that should be allowed in any given case of diminished responsibility. That is a question for the jury alone to decide. While we admit that in some respects our criminal law needs amendment, it must not be forgotten that the semi-barbarous social condition of our people renders the administration of even-handed justice impossible in many cases. Until we provide security that the criminal insane shall be permanently placed where they shall be permanently harmless to society, we must expect that the plea of weakened responsibility will often fall upon deaf eared jurors, and that medical experts will often seek to ascertain how much responsibility, rather than how little, can be established in any case that is submitted for their opinion. These unfortunate social conditions do undoubtedly render more difficult than it should be the task of the conscientious medical witness, but they can not be urged in justification of a practice that exists in our law courts, of introducing medical testimony merely for the purpose of raising unfounded doubts in the minds of ignorant jurors.

A medical witness should be free to comment upon testimony and, if necessary, to show its insufficiency; but he should not be licensed to befog the minds of jurors by the expression of hypotheses (guesses) for which there is no possible foundation in the evidence before him. In many instances the opinion of a medical witness can not be the expression of absolute certainty, but is merely the utterance of an inference based upon a necessarily incomplete presentation of facts. In all such cases the expert must remember that he is dealing only with probabilities, and he must take care that his inferences are the legitimate and logical consequences of accessible facts, and that they be not colored and vitiated by any inclusion of unproven possibilities. The neglect of this precaution has sometimes led to flagrant injustice.

For the reasons thus briefly set forth, I am not inclined to expect much benefit from any proposed change in the employment of medical expert testimony. Human nature and the mode of legal procedure must be improved, before any real advance can be made in this matter. Fortunately, we are not responsible for the defects of human nature, nor for the imperfection of the courts of law. Let us attend to our own affairs, and improve the quality of our experts; for in this way we may accomplish something of a substantial character for the benefit of society.

LECTURE ON DRINKING WATER.

AN ABSTRACT.

BY CHARLES SMART, M.D.
SURGEON U. S. A.

A lecture on drinking water was delivered by Dr. Charles Smart, U. S. Army, on the 5th inst., under the auspices of the Sanitary League of Washington, D. C. The lecturer spoke first of waters generally and their constituents, dividing the latter into inorganic matters, dead organic matter and living organic matter. The first of these he represented as not necessarily a sanitary impurity, for the human system required and assimilated from articles of food and drink over an ounce of just such salts daily. If a water is saline to the taste, or alkalin or chalybeate, or is so hard as to curdle the soap in washing instead of forming a foaming lather, it will probably cause intestinal disorders. Otherwise the analyst has little interest in the inorganic salts as the totality of their amount is insufficient to occasion harmful effects. The dead organic matter is of importance because it was once alive and because it is likely to be associated with that which is still alive. It is not specially dangerous in itself,—in fact it is seldom harmful unless present in quantity sufficient to give a taint or odor to the water. Nor do the living organisms ordinarily seen in the sediment of a water have much bearing on the quality of the water.

"You will observe, then, that since the saline matters and the dead organic matter are not specially unwholesome except when present in quantities appreciable to the taste, and since most of the living organisms present in a water have little influence on its quality, the question of wholesomeness becomes reduced to a determination of the presence or absence of certain organisms which we know to be the germs of disease and to be sometimes contained in water supplies. These are the germs of malarial and typhoid fevers and of cholera. It is to the microscopist and bacteriologist that we would naturally look for the detection of these, but bacteriology is yet in its infancy—a lusty infant, it is true, and giving promise of a phenomenal manhood. There may be in the distributing reservoir of a large city enough of the germs of typhoid fever to cause sickness and sorrow in many a household in the course of a year, but what chance has the microscopist of discovering them in the part of a drop which covers the field of his microscope? There may be several specimens of four-leaved clover in the meadows, but what are our chances of finding one in a handful plucked at random?

"Extravagant ideas of the powers of scientific in-

vestigators are sometimes published. Not long ago, I saw it asserted that a cholera germ had no more chance of passing along the current of the Potomac undetected by the bacteriologist, than a mad dog had of careering along Pennsylvania Avenue at midday without being discovered. Indeed, it is because of the inability, so far, of the bacteriologist to detect the germs, that an appeal for information is made to the sanitary chemist. The chemist examines the dead organic matter in the water. Malarial infection is a product of a soil in which the decay of vegetable organic matter is in progress. Waters draining from such a soil carry with them the malarial infection, and also more or less of the decaying organic matter. The presence of the latter, discovered by the chemist, suggests the likelihood of the concurrent presence of the former. When the germs of typhoid fever or of cholera are present in a water, they generally enter the water along with sewage, for it is known that these pathogenic or disease-breeding organisms are cast out of the sick person by way of the intestinal canal. Hence, when the presence of animal matter or sewage is detected by the chemist, the likelihood of the concurrent presence of the bacillus of typhoid fever is suggested. Chemistry makes no pretense of discovering the germs of disease, but merely suggests the likelihood of their presence—the presence of malaria in association with vegetable matter; the presence of the bacillus of typhoid in association with animal matter."

The lecturer then divided waters for his sanitary inquiry into: Rain or cistern waters, surface waters, subsoil or ground waters and deep or subterranean waters. The first he showed to be pure and wholesome when ordinary care is taken in their collection and storage. The second, or surface waters, are pure or impure according to the nature and condition of the shedding surface. He instanced the Rio Grande as a malarious surface water, and the cause of the malarial fevers which rendered Fort Brown, Texas, the most insalubrious of the United States military posts until a new and pure water supply was provided, when with the discontinuance of the river water the post became as healthy as an Atlantic summer resort. The epidemic at Plymouth, Pa., in 1885 was brought forward as an illustration of typhoid fever propagated by surface waters.

"Local outbreaks of typhoid fever have been traced so frequently to the use of the water of shallow wells that the health officers of most of our municipalities have interdicted their use. But on the other hand, if the soil is fresh and unpolluted by the filth that is unavoidably associated with a long continued occupancy a free supply of soft wholesome water may be obtained. Brooklyn, Long Island, is supplied in part by sub-soil water piped from a clean filtering area at some distance from the city. At isolated country houses the dangerous character of the shallow well arises from the proximity of cesspools, pits, garbage heaps and manured ground; and not unfrequently the handsome trees that throw a grateful shade over the cool waters of the well form channels of entry for unfiltered seepage along the course of their roots. The distinguished bacteriologist, Prof. Koch, attributes any unwholesomeness of such wells to unfiltered inflow from the surface, but whether the contamination is from unfiltered inflow or from the inefficiency of the subsoil as a filter, the facts remain that such waters are found by the chemist to contain much organic matter, vegetable and animal, and by medical observers to be associated with the propagation of both malarial and typhoid fevers.

"The deep or subterranean waters are those that falling on high ground sink into broken edges of some porous stratum which underlies the clay beds of the bottom lands. The waters percolate through and along the porous stratum underneath the impermeable clay which supports the subsoil water. When tapped by digging or boring they are the waters of our deep wells, originally pure, but often valueless for domestic or manufacturing purposes on account of the large amount of mineral salts which they have dissolved in their subterranean course. If the pervious layer be one of sand and gravel the water may be as pure as distilled water. The discovery of such water 350 feet below the surface of the city of Memphis, Tenn., solved the water question for that city at a time when its citizens were debating whether the million dollars which they could raise for a water supply should be expended in bringing in the clay-laden waters of Wolffe River or the sewage-polluted waters of the Mississippi. The thorough filtration to which this deep-lying water is subjected in its subterranean course freed it from all germs, whether of malaria or of typhoid."

Dr. Smart then gave some interesting practical illustrations of the methods followed by chemists in testing waters to discover the presence of various kinds of impurities.

The simplest single experiment employed by the chemist is the charring or burning of the residue after evaporation. If there is merely a slight darkening without fumes or odor the water is free from organic matter. If there is much blackening which is dissipated with difficulty and with the evolution of peaty or pyroligneous odors the matter is vegetable. If there is a strong nitrogenous odor and the carbonaceous blackening disappears with incandescent sparkles and nitrous fumes the matter is animal. This is but a rude and primitive experiment but it gives valuable information in the absence of facilities for more elaborate methods. The organic purity of a pure water may be demonstrated by it, as well as the impurity of a notably bad water.

Purification by sedimentation was then discussed, as it is by this process that the Potomac River supply of Washington is supposed to be treated. The inefficiency of the local arrangements was pointed out and the high death rate of the city from typhoid fever was attributed to this inefficiency. "The greater the care taken in the purification of a city's water supply the less is the death rate of that city from malarial and typhoid fevers. Brooklyn has an annual death rate from typhoid fever of 15 in every 100,000 of its population; New York 26; London 28; Baltimore 40; Boston 45; Cincinnati 63; Philadelphia 66; Chicago 69, and Washington, according to Dr. Billings' recent census work, 75. We do not have this high rate because our sewerage system is defective, for Baltimore has no sewerage system, nor because our houses are overcrowded and unventilated, back yards filthy and undrained and streets narrow and unclean. No. We have it because the germs of the disease are distributed along with the clay particles of the Potomac water. The inflow of the sewage into the Potomac River above the Great Falls is small, but there is enough of it to give us more typhoid fever than is found in Baltimore, New

Orleans and other cities that lack many of our sanitary advantages."

The Army engineers in charge of the water supply are aware of the defects in the system, and have excellent plans for remedying them; but money is required to carry them to completion. Dr. Smart closed by urging the expenditure.

"Think of the 75 deaths from typhoid fever annually in every 100,000 of the population, 60 deaths more than in the city of Brooklyn, and more than we would have in this city if our water was as pure as that of Brooklyn. Putting our population at a quarter of a million, we have 150 unnecessary deaths every year. Ah! but the cases! To find the number of cases we must multiply the deaths by twenty! Three thousand cases of serious sickness, each lasting from two to six weeks. Three thousand cases of disease that might be prevented by the expenditure of a little money. Think of the suffering, anxiety and sorrow involved in the history of these three thousand cases, and then say whether an appropriation having in view the purification of the city water supply is a good investment or a bad one."

DESCENT OF THE TESTICLE IN ADULT LIFE.

BY ALBERT H. TUTTLE, M.D.
CAMBRIDGE, MASS.

As such cases as the following must be of extreme rarity, and as they shed a great amount of light on the phenomena of earlier and normal descent, and at the same time perhaps instruct us somewhat as to the manner in which we should deal with cases of undescended testicle; cases where the majority of opinion has been in favor of the complete removal of the organ when it interferes with the comfort of the patient, is caught in the inguinal rings, or complicates a hernia, I have deemed this instance one worthy of consideration.

Not long ago I operated for strangulated hernia complicated with undescended testicle, and after closing the hernial sac and rings, drew the testicle, which was enclosed in the sac, into the scrotum and fastened it there with the tunica by means of stitches. This procedure necessitated the invagination in part of the scrotum. The result was perfect, but after the operation the gentlemen present, some four members of the profession, expressed as their opinion that I should have sacrificed the testicle, and questioned the result. When they said such testicles do not prove of much practical value, I could only answer that I had been consulted by patients with one undescended testicle who were willing to undergo a surgical operation in order to satiate their desire for a second testicle in the scrotum. They simply echoed the general opinion of the profession. But the time has come when we must deal more rationally with these cases, and it is already heralded by various reports in our latest journals.

E. W. L., aged 31 years, was born with a congenital hernia on the right side and an undescended testicle on the left. He wore a truss on the right side until 12 years old; it was then discarded until the age of 21, when he put on a double truss and wore it until six months ago; since, he has employed simply a suspensory bandage to keep the bowel in place.

I saw him for the first time about two years ago; an examination showed an absence of the left testicle, and a dilated condition of the inguinal rings on both sides. In the standing position the abdominal wall bulged outward at the site of the rings, and an impulse was felt on coughing. It was obvious that the sac was not a great one, and that the gut at that time would not descend far along the cord, although it had at an earlier date, according to the statement of the patient. He consulted me for an attack of gonorrhea at this time, and informed me that he had had a similar attack some six years previous. During his first attack he had a swollen testicle on the right side and sharp pain deep in the left groin. Since, he has had a swollen testicle on the right side without any apparent cause. I recognized the condition of the patient at the first visit, and recommended an operation for the radical cure of the herniæ.

He went to Europe shortly afterwards, and I saw nothing of him until a few weeks ago. I now find the testicle on the left side of the scrotum, the inguinal rings smaller than when I first examined them, and, without artificial support, the gut does not distend the abdominal wall or give an impulse on coughing to the contents of the scrotum. The patient experiences no discomfort without the truss, and only about once a week a "lump" appears in the left groin "in the evening when the muscles are tired;" this disappears on lying down, or on pressure.

The following history of the descent of the testicle is given in the words of the patient as nearly as possible: About a year and three months ago a skin, like a bag full of water, came down, and kept getting larger toward evening; the bag became about egg-size, and could be emptied by pressure; in about three months—a year ago—he was sitting writing a letter in the evening, when something was suddenly felt below (in the scrotum); he looked and found the testicle had come down. It disappeared the same evening, but returned to the scrotum a week later, when he tied it down with a piece of tape. In this manner he kept it down for a week; it then returned to the abdomen, and remained concealed three days, and when it came down stayed longer. It has kept going up and coming down, each time staying longer in its normal condition, until now it has remained in the scrotum for about four months.

When the "bag of water" first appeared, it made the scrotum red and hot and somewhat uncomfortable; it disappeared during the night, and on lying down.

He has been married for about one year. Since the attack of gonorrhea two years ago, he has had at times bloody urethral discharges with the seminal ejections, or immediately after intercourse. Examination by the microscope shows nothing abnormal in the seminal fluid except the presence of blood, which probably mixes with it somewhere in the genital tract, external to the testicle. This bloody discharge, for which he recently consulted me, is a complication of the old gonorrhea, and in my opinion has nothing to do with the lately descended testicle.

It seems possible that by care this case will obtain a spontaneous cure of the herniæ.

TRACHOMA.

Read before the South Kansas Medical Society at Wichita, Kan., Nov. 21, 1893.

BY FLAVEL B. TIFFANY, M.D.
PROFESSOR OF OPHTHALMOLOGY IN THE UNIVERSITY MEDICAL COLLEGE OF KANSAS CITY, MO.; OCULIST TO GERMAN AND ALL SAINTS HOSPITALS, ETC.

Trachoma (from the Greek τραχυς, rough), is the term intended to designate a certain disease of the lining of the lids or palpebral conjunctiva; but it has been so indiscriminately used by physicians and some oculists when speaking of the different forms of conjunctivitis that there is much confusion as to its real signification. To most physicians, it means any form of granular conjunctivitis, be it simple, acute, subacute or chronic; especially if there is much roughness of the lining of the lids. In acute conjunctivitis we have a swelling of the papillæ, giving a very rough condition of the palpebral mucous membrane, but this is not trachoma.

In catarrhal ophthalmia, and especially in purulent ophthalmia, the papillæ swell enormously, also the different glands, those of Moll and the Meibomian as well as the trachoma glands; but again, this is not trachoma. Inflammation of the conjunctiva may pass through the different stages, from simple

hyperemia, acute, catarrhal, purulent, subacute and chronic even; and yet not reach that stage or condition which the word, trachoma is intended to signify. It is believed by most authorities that trachoma is consecutive to the different forms and stages of inflammation of the conjunctiva; and according to my own observations and study I am inclined to believe that this view is the proper one. It is, as a rule, a disease confined to the common or lower classes, especially to those who are exposed to irritants and subjected to bad air and other poor hygienic conditions. It is particularly a disease of the so-called scrofulous people; and of the offspring of parents of blood relation; in other words, it is often due to consanguinity.

Of the different kinds of conjunctivitis we have acute, subacute and chronic; we have also what is known as catarrhal and purulent ophthalmia, any or all of which may be precedent to trachoma; that is to say, if these inflammations are permitted to continue through the consecutive stages, they may finally reach or produce the condition of true trachoma. Acute catarrhal ophthalmia is characterized by a turgescence and swelling of the papillæ with more or less congestion and swelling of the whole palpebral conjunctiva. The papillæ may become quite hypertrophied, turgescent, red and succulent; the conjunctiva itself is thickened, especially in purulent or in pronounced catarrhal ophthalmia. In these latter diseases the conjunctiva is so swollen and congested as to be thrown into folds or rugæ, and the adenoid tissue assumes a velvety appearance. The turgescence and hypertrophied papillæ are usually more pronounced at the retrotarsal folds, especially near the canthi. Sometimes the papillæ with the mucous membrane are so enormously swollen and engorged as to be thrown into such large folds and excrescences they resemble a cauliflower; and when the lid is everted, it resembles, as Dr. Noyes has said, the everted rectum of a horse after defecation.

In the simple acute conjunctivitis there is little or no purulent discharge, although the excrescences are frequently so turgid as to be made to bleed at the least provocation.

In catarrhal and purulent conjunctivitis a similar condition exists, but the nature of the disease is different. In acute conjunctivitis there is a slight discharge, but not of a purulent character; it is more of a mucoid nature. The follicles of the conjunctiva are usually also swollen and give the appearance of fish spawn or of tapioca grains; the papillæ, however, frequently rise above the follicles and completely hide them. In subacute conjunctivitis the follicles are more pronounced and the papillæ are less hypertrophied. The granules in the subacute form are frequently more prominent upon the lower lid or may be almost entirely confined to the lower lid, where the fish or frog spawn or tapioca appearance is to be seen. The chronic form, or what is usually called trachoma, is quite a different condition from the other two; in this form we not only have a hypertrophy of the papillæ, the follicles and trachoma glands, with the glands of Moll, but we have true neoplasms of follicular infiltration. The lymphatic stomata are filled with leucocytes and follicular deposits. The infiltration of leucocytes into the stroma and the adenoid tissue of the lid assume a true granular condition with connective tissue cells or nuclei surrounded by connective tissue fibers and atrophy of the normal connective tissue. *In real trachoma we have, then, hypertrophic succulent papillæ, hypertrophic follicles, hypertrophic glands of Moll, hypertrophic trachoma glands and, besides, neoplasms of a granular character predominating. There is an infiltration of connective tissue substance throughout the conjunctiva. The normal stroma becomes atrophied and, to a certain extent, gives place to the neoplasms similar in appearance to the ordinary tubercle of phthisis.* Within these cells or nuclei of the neoplasm, have been found numerous microbes called trachoma bacilli.

Chronic conjunctivitis or trachoma is thought to be contagious. Frequently this disease develops seemingly quite independent of the acute or subacute form of conjunctivitis, and exists for many months before it is discovered.

The different forms of conjunctivitis, if not timely and properly treated, may assume a chronic condition and this condition by some has been called trachoma. Trachoma, however, is something more than this. As before said, in trachoma we not only have hypertrophy of the papillæ, of the glands of Moll, the Meibomian glands, a swelling and thickening of the whole conjunctiva; but we also have follicular infiltration into the mucous membrane and the adenoid tissue; we have a breaking down of the normal tissue, a derangement of the histologic structure with atrophy, with cicatricial tissue and the follicular neoplastic infiltrations resembling tapioca or sago grains.

Conjunctivitis in other forms can be cured by proper treatment in a short time, but it is far different with true trachoma; here the disease is most persistent, frequently resisting any and all treatment, marching on its destructive way for months, even years, before it can be subdued or scarcely checked in its course. Some authors will tell you of several forms of trachoma; but trachoma is trachoma wherever you find it. That there are different stages and exacerbations we grant. That there are different complications existing we well know. In some cases the papillæ are much more congested and hypertrophied than in others; again, in some cases the follicular infiltration is so enormous that the neoplasms stand out in such bold relief as to resemble cauliflower growth. *The chief distinguishing feature, then, of trachoma is the cicatricial formation and neoplastic follicular growths*, and this seems to be a consecutive outgrowth from the acute to the simple catarrhal or purulent, to the subacute, and thence on to the chronic. Whether there is a specific microbe that comes in now to play his part is a question. Others claim that the disease is of a specific nature due to a particular microbe, and with this bacillus or micrococcus they can inoculate the lower animals, and produce there a true trachomatous inflammation. We all well know that the conjunctival sac swarms with many forms of microorganisms, but it would be difficult to say which, if any one, is responsible for the disease. Nearly every oculist who has claimed to have found the bacillus or micrococcus producing trachoma has a microbe differing from all others. As yet, it is wholly speculative whether the disease has its etiology in any specific organism or germ.

One of the most frequent provoking causes of trachoma is some error of refraction, especially that of hypermetropia or hyperopic astigmatism. In my experience, the majority of these patients are hyperopic, and

many are astigmatic. There is another factor that comes in as a cause, and that is the use of tobacco, especially if chewed; and I have found that patients having chronic granular conjunctivitis or trachoma, can not be cured so long as they continue the use of tobacco.

It would seem that the swallowing of the juice which all chewers of tobacco do to a certain extent, poisons the lymphatic system, and so affects the lymphoid and adenoid structures, and until this poisonous nicotine is entirely eliminated from the system it is next to an impossibility to effect a cure. So confident am I that tobacco thus affects the system and prevents the cure of these cases that I absolutely refuse to take a patient using the weed who will not promise to discontinue its use in toto. I have made a careful record of all my patients, and have given special attention to this subject of tobacco, and its effect when used by the patient or having been used by his progenitors; and I have found that a large per cent. suffering from persistent trachoma either use tobacco or their immediate ancestors did before them; and I have also satisfied myself that patients who are not contaminated by the weed get well in a much shorter time than those whose system is under its influence.

I have also experienced that patients suffering for many months and years with trachoma, having received the most approved and continued treatment from the hands of competent physicians and oculists, are cured by the ordinary treatment in a very short time, as soon as they discontinue the use of tobacco.

We know that it is a fact that this disease, trachoma, is more often found among the Irish and American people than among those of other nationalities, and we also know that it is a fact that the chewing of tobacco is limited almost entirely to the Irish and American people.

That trachoma is contagious there is some question, but that it is infectious, there is no doubt.

SYMPTOMS OF TRACHOMA.

The patient complains of a gritty feeling beneath the lids as though there were sand or other foreign bodies lodged there. There is more or less dread and intolerance of light, the photophobia varying in intensity according to the stage and intensity of the disease. The secretion glues the lids together, especially in the morning after sleep, forming a yellow crust at the roots of the lashes. Frequently there is paresis of the levator palpebræ superioris, and in consequence, more or less ptosis (falling of the lid). Sooner or later the cornea is almost sure to be affected, when the ocular conjunctiva is also involved. There is turgescence of the ocular conjunctiva with, perhaps, the blood vessels of the subconjunctiva extending into and upon the cornea with more or less swelling of the margin of the cornea; and this vascular opacity of the cornea is called *pannus*. The pannus, as a rule, begins at the upper part and the vascular opacity may extend upon, midway or even across the cornea, completely obscuring the iris and pupil. This pannus is due to the friction of the rough granular lid and it begins at the upper part of the cornea where it may be confined, for it is usually the upper lid that is mostly involved, and as there is much more motion of the upper lid upon the ball than of the lower, this adds to the corneal complication. It is not infrequent, however, that both superior and inferior lids are granulated at one and the same time, but usually the upper one is far more diseased. The irritation of the rough lids upon the delicate cornea often produces ulceration here, and any existing pterygium is aggravated and may rapidly increase.

PROGNOSIS.

Acute conjunctivitis may run its course in a few weeks and spontaneously get well, but if neglected or improperly treated it may pass into a chronic form, when months and years may be required to effect a cure; and even then with much impairment, if not loss of vision, from some one or many of the complications and sequelæ of the disease. From the infiltration there is atrophy of the stroma with cicatrices; the cul-de-sac or pouch formed by the conjunctiva as it folds over from the lid upon the globe, and which is so necessary to the freedom of movement of both lid and eyeball, becomes obliterated; that is to say, instead of the conjunctiva folding back and forming the cul-de-sac the two surfaces, the palpebral and ocular, are grown together, and the membrane of the lid passes directly to the eyeball. This condition not only interferes with free movement of the lid and ball, but it also tends to contract the lid and invert its free edge, thus drawing the lashes down upon the ball, or even turning the edge of the lid with the lashes so much as to completely fold them in against the palpebral conjunctiva and upon the eyeball; in other words, produce what is termed *entropion*, which condition is most harassing and destructive to the cornea. This entropion is also further favored by atrophy of the intra-marginal space of the lid, thus allowing the ciliæ to drop down and change their normal outward curve to an inward direction. If the disease is seen before these sequelæ arise, and timely and proper treatment judiciously given, it can be cured with little or no damage to vision.

TREATMENT.

In simple acute conjunctivitis, the treatment consists in looking after the hygienic condition of the patient; after his general health; and removing or rectifying any provoking cause, as of a foreign body, or any anomaly of refraction. See that the general health is up to the standard; that food, drink and clothing are good. Tonics, if necessary, should be given; however, it is frequently the case that we find this disease among those of robust plethoric people otherwise in perfect health with ravenous appetite; in fact, the majority of my patients suffering from granular conjunctivitis are from the country, and are well nourished and full of blood. In these cases, it is best to diet them, and I have frequently found that depletion is beneficial. As a local treatment, hot water should be freely used. As a collyrium, the chlorid of zinc is my favorite, ½ to 1 grain to the ounce of water, dropped into the eye night and morning; atropin ½ per cent. to 1 per cent. used once a day, if not continued too long, does good. Other astringents, as sulphate of zinc, alum, borax, sulphate of copper, etc., may be used with equal efficacy. If the disease has assumed the subacute stage, more vigorous treatment may be necessary; here the lids should be everted and the mucous membrane bathed with a solution of nitrate of silver, 1 to 2 per cent., every other day, and in the meantime a solution of boracic acid in warm water 8 per cent. (a

saturated solution being 33 per cent.), can be used three times a day, the patient bathing the lids by means of absorbent cotton saturated with the solution, allowing some of the fluid to go into the eye. A little vaselin, plain, or with yellow oxid of mercury, 1 per cent., should be smeared on the margin of the lids at night to prevent them from gluing together. If the disease has gone on to the chronic form or true trachoma, we then have a far more formidable affection to contend with.

TREATMENT OF TRUE TRACHOMA.

Trachoma frequently baffles the most skilled and approved treatment, and goes on for months and years even before it is thoroughly subdued. At one time it was thought by some as incurable, for if once seemingly subdued, when treatment was discontinued the disease would surely return. From its rebellious character and destructive sequelæ it has compelled the profession to give it much attention. Nitrate of silver solution and sulphate of copper in crayons are the two remedies that have been found most efficient, and in ordinary cases they are most to be relied upon. Nitrate of silver may be used as strong as 10 per cent. or 15 per cent., but it is a question in my mind if such strong solutions do more good, or even as much, as weaker ones; besides, if they come in contact with the cornea, they are liable to do much damage to this delicate structure. The strength that I have found most satisfactory is from 2 to 4 per cent.; that is, 10 or 20 grains to the ounce of water, brushed on to the everted lids two to six times a week. In using the strong solution of nitrate of silver, it should be applied by means of absorbent cotton wrapped around a wooden toothpick or any applier dipped into the solution, and after the solution has remained a second or so, until the mucous membrane begins to turn white, the excess should be washed away by plain cold water. Care must be taken not to allow the solution to come in contact with the cornea, lest it provoke ulceration of this delicate bloodless structure. No solution of nitrate of silver stronger than ¼ per cent. or ½ per cent. should be dropped into the eye. If the sulphate of copper is used, it is best applied as a crayon, and the pencil must be perfectly smooth, for if not, the rough surface will irritate and do harm instead of good.

In treating trachoma, care must be taken that the application be within the retrotarsal folds as well as upon the palpebral conjunctiva. The point of the copper crayon can be passed quite into the cul-de-sac. The copper should not be used oftener than three times a week. If it is dipped into vaselin or glycerin before applying it, it is not so painful, but quite as efficacious. The sulphate of copper treatment is very painful, and in sensitive people, cocain may be used previous to the treatment; however, I have found that the reaction of cocain is bad, and if it is long continued, it is decidedly prejudicial. If there are any abrasions, ulcerations, or pannus of the cornea, sulphate of copper should not be used, as it will most likely aggravate these corneal diseases. Here nitrate of silver is far more beneficial. The yellow oxid of mercury salve, five or six grains to the drachm, is a favorite treatment of the European oculists, but I have never been able to use it so strong on my patients without the most excruciating pain, with intense irritation, causing the eyelids and ball to be all aglow; indeed, four or five grains to the ounce of vaselin is as strong as my patients will tolerate, and if I use it stronger, I have to combine cocain with it. The formula I mostly use at my clinic is ten grains each of cocain and yellow oxid of mercury to the ounce of albolene. I have the mercury thoroughly ground in a mortar until there are no particles visible, it being thoroughly mixed with the albolene, for if but a very small particle exists not comminuted in the albolene, it will prove an unbearable irritant. The salve seems to be less active after it has been made for a time than when it is first put up. I frequently use it alternately with the silver or copper treatment. Nitric acid is sometimes used, but it is dangerous and is liable to leave eschars and cicatrices, thus irretrievably injuring the lids and eye; if used at all, it should be applied by means of a pine stick dipped into the acid. After the acid has soaked into the stick, the latter may be cautiously drawn across the granules. Charlatans used this treatment frequently, subduing the granules, but as frequently permanently injuring the parts by the cicatrices left.

In pronounced chronic trachoma the most speedy and radical treatment is that of expression of the granulations either by the thumb nails, the trachoma forceps, or toothbrush. In using the trachoma forceps Dr. Knapp's pattern is best; some prefer Prince's. The patient should first be anesthetized, as the operation is so painful that cocain is insufficient. The lids everted, the forceps are applied by shoving one stirrup beneath into the cul-de sac, the other at the everted margin of the lid, and with the forceps firmly compressed careful traction is made. Care must be taken not to tear the tissues, only sufficient force being exerted to roll out or scrape off the granules. It is the cauliflower growth, excrescences, and hypertrophied papillæ and glands that we thus remove with some of the neoplasms. I have found that it is necessary to make subsequent treatment for weeks and months after the use of the forceps, even after the most thorough use of the instrument; but certainly they cut short the disease as, by their use, a few weeks, generally with application of nitrate of silver or copper, will accomplish as much as could be gained in as many months by the usual treatment without their use.

Of very recent date I have used the stiff brush, (toothbrush). After first removing all the granulations possible by means of the forceps, I then take the toothbrush (cut down short), and with the lids everted thoroughly brush the conjunctival surface until all neoplasms and hypertrophies are removed (the lids appearing thin and pliable). There is not as much reaction from this treatment as one would suppose; but the results are not always satisfactory, for even with the brush in addition to the forceps, I have found it is still necessary to follow up this by a slight astringent and in some cases, from the brush, I have had with the cicatricial contraction, pronounced entropion. In case of pannus, which frequently persist even after the trachoma is cured, syndectomy, as my experience proves, is the most satisfactory treatment; this consists in making an incision around the cornea through both conjunctiva and subconjunctiva about three or four mm. distant from the limbus, and then dissecting the intervening portion up from the sclera to the margin of the cornea and snipping it off by means of the scissors. This girdling the cornea, as it were, cuts off all the

blood vessels of the cornea. The vascular opacity of the cornea immediately diminishes and rapidly disappears, also the neoplastic hypertrophied tissue of the cornea is quickly absorbed and the cornea gradually clears up. The conjunctiva recedes from the limbus upon the globe, the tension is relieved, thus allowing the retrotarsal folds to spring back and the margin of the lids to recover their normal position.

Jequirity.—In very chronic obstinate cases of opacity, *jequirity infusion*, after all other means have been exhausted, may accomplish still more good; but it should be used with caution, as it sometimes produces ulceration of the cornea. It can be used with more safety after syndectomy has been made, for if there be much swelling of the conjunctiva, even though it has been re-attached to the sclera, its attachment will give way and so prevent any pent up pus or strangulation of the cornea.

Inoculation—of purulent conjunctivitis is too dangerous to be recommended, except in rare cases where, perhaps, there is dense, persistent, vascular opacity with plastic infiltration of the cornea, with chronic trachoma; and even then, if the malignant inflammation caused by the inoculation is not carefully guided, it is liable to cause ulceration and destruction of the cornea; but the danger is much lessened if peritomy or syndectomy has previously been made; for as the conjunctiva becomes infiltrated with intense chemosis, the incised periphery near the cornea is loosened, thus avoiding the pent up secretion or engorged vessels, and hence lessening strangulation of the cornea. If there be strangulation, thus impairing the nutrition of this vulnerable part of the eye in its enfeebled state, it is in great peril, for the deadly specific microbe burrows and chisels its way into the cornea, thus causing abscess and sloughing of this part of the eye, for it is the weaker and devitalized tissues always that these germs prey mostly upon. And so I repeat that the operation of syndectomy or peritomy indemnifies or insures the cornea to a certain extent against the destructive force of the inoculation. The effect of the purulency by inoculation is frequently so very striking, clearing as it does, the opacity of the cornea and ridding the lids of all infiltration and granulation, that it is very seductive and alluring; but if one has ever tried it, wrestled with it days and nights, as I have, to prevent its destroying the eye and finally at last having suppuration of the cornea, he will not feel like repeating the experiment.

I remember years ago having a case of most persistent chronic trachoma with dense pannus and infiltration of the cornea which for months and years resisted all treatment. I finally screwed up my courage to try gonorrheal pus. I inoculated one eye and soon an intense inflammation followed; the lids swelled enormously, became fiery red and pus flowed out upon the cheeks; the boy, a Swede of 14, screamed with pain; soon the other eye was attacked and the lad fairly howled with agony, creating a general consternation in the neighborhood. I went to see him several times a day and during the night for a period of more than a week, expecting every hour that the eyes would burst; but my relief of mind and my joy, you may well imagine when the inflammation began to subside without any destruction of the cornea. In this case I had previously made a syndectomy, and although the conjunctiva had attached itself to the sclera and the intervening space was replaced by new tissue, during the intense swelling and chemosis, the conjunctiva gave way again all round the cornea, allowing free vent to the secretion and there was no strangulation. To this I attributed the success of conducting the suppurative inflammation to its happy issue. In this case, the cornea cleared perfectly and the lids were rid of all granulation, and the boy regained good vision with splendid looking eyes.

This was, and probably will be my only case, unless it be as a last resort, after long and faithful trial of all other means, and even then when there is no vision, where there is nothing to lose but everything to gain. However, the jequirity will accomplish nearly as much and with much less danger.

The form of jequirity to be used.—Take eight beans, crush or grind them, and macerate in an ounce of warm water for twenty-four hours. Strain, put up in blue bottles, and keep in a cool, dark place. Of the solution, instill one or two drops into the conjunctival sac, or if a more thorough application is desired, it may be made by everting the lids, and applying the solution by means of absorbent cotton. The effect of the drug thus applied is not manifested until five to eight hours when the symptoms of catarrhal ophthalmia begin. The inflammation continues to increase when it assumes more of the symptoms of purulent ophthalmia, there being more or less suppuration of a muco-purulent nature. Soon there is a false membrane formed not unlike that of diphtheria. If the inflammation thus exerted proves too severe, it may be held in check by the use of hot water; otherwise, no further treatment is necessary, except to keep the lids cleansed by means of absorbent cotton and warm water.

The jequirity infusion will spoil in a short time, especially in warm weather; therefore a fresh solution is necessary. Parke, Davis & Co. keep a solution on hand which I have found quite reliable. This solution is strong, but may be reduced by adding a few drops to as many more of water before applying. If a prolonged action of the drug is desired, one drop of the reduced solution may be instilled into the conjunctival sac daily or every other day for a week or ten days when the eye should be allowed to recover entirely from the effect, and then after an interval of a few weeks the solution may be repeated if necessary.

In treating trachoma we should look well after the hygienic condition of the patient and also to existing provocative influences, such as the errors of refraction, especially hyperopia and astigmatism, also the different forms of heterophoria. Very frequently there is some disease or obstruction of the lachrymal apparatus that has to be relieved or treated ere a cure of the trachoma can be produced. Frequently have I effected a speedy cure of trachoma that had been resisting the ordinary treatment for many months, by simply slitting up the canaliculus and passing a probe into the nasal duct, thereby securing a free drainage of the pent up diseased fluids, relieving the irritation caused by their presence.

Tobacco in all forms must be prohibited in toto; also alcoholic liquors, except when indicated as mild stimulants. General baths give tone and health to the system and are always indicated.

Blank Applications for membership in the ASSOCIATION at the JOURNAL office.

ON PRESERVING EMBRYOLOGICAL MATERIAL.

BY ADOLF MEYER, M.D.

DOCENT IN NEUROLOGY, UNIVERSITY OF CHICAGO; PATHOLOGIST OF THE ILLINOIS EASTERN HOSPITAL FOR THE INSANE, KANKAKEE.

Preparing some work on the development and the anatomy of the brain, I beg to bring the following appeal for coöperation before the members of the profession:

It is necessary to start the work on as broad a basis as possible, and for this reason it can not be done without the active coöperation of the practitioners. It is my desire to show that a large city like Chicago can, notwithstanding its short existence, make up for a long period of preparation with its youthful energy and take at once, as it did in many respects through the Fair, a place in the foremost ranks of scientific centers. This energetic progress shows itself in every line of scientific research, and I feel confident that an appeal for help in promoting our knowledge of the nervous centers will not be in vain.

Embryological material becomes of value only if a sufficient number of stages are collected and displayed in an instructive series of development. Such a museum will, I hope, be one day the pride of Chicago and furnish the best ways of instruction, far superior to book learning and drawings. The preparation of the material is not connected with much inconvenience. It is, however, necessary that certain rules should be observed, which increase the value of the specimens materially.

Very early stages which are sometimes found in the blood-clots of "very profuse menstruations" are best put into a 10 per cent. solution of nitric acid for a quarter of an hour and then into an abundant quantity of 70 per cent. alcohol. If the fixation with nitric acid is not possible, it will be quite sufficient to put the ovum at once into the 70 per cent. alcohol.

This method will answer its purpose up to the fourth month. From the fifth month, however, it will be preferable to preserve the material in Müller's fluid (bichromate of potash 2½ per cent. and sulphate of iodium 1 per cent. dissolved in water). This method is somewhat more complicated, inasmuch as it requires a little more attention. The fetus—in later stages only the head and perhaps the spine—is immersed in a large quantity of the fluid. It is best to open carefully the cranial cavity in order to give direct access to the fluid; the removal of the parietal bones or at least their partial removal will answer best the purpose. On the second and fourth day the fluid should be changed. Under all circumstances the fluid must be abundant.

Preference is given to Müller's fluid in the later stages of embryonic life and in the new-born child, because not only the surface anatomy (convolutions, etc.), is of importance in these stages, but especially the development of the medullary sheaths of the fiber tracts which takes place at different times and affords a most useful help for the study of special tracts.

With regard to the jars, preserve jars of an appropriate size can be obtained in every grocery store. It will not be inconvenient to keep a 5-gallon jug of Müller's fluid and a gallon or two of 70 per cent. alcohol ready all the time.

The specimens should be accompanied by a short note on time of last menstruation, time of beginning abortion or labor, and time of expulsion and further the time when put into the hardening fluid. All the specimens will be duly acknowledged.

It is evident that monstrosities offer much of interest with regard to the nervous system. The same would hold for pathological conditions in children and adults. With regard to the transportation, specimens may be sent for when brought to one of the Chicago down-town offices, or they may be sent to the Kankakee Hospital by express.

Kankakee, Ill., Jan. 27, 1894.

DISLOCATION OF CERVICAL VERTEBRA. DEATH—CASE RECORD AND NECROPSY.

BY G. W. BROOKE, M.D.

ELLSWORTH, OHIO.

Early on Wednesday morning Nov. 29, 1893, I was hastily summoned to visit a neighbor, Mr. Chester Allen, a strong, healthy man, age 50 years, living but a short distance from my residence. I found him in his barn, and learned that he had fallen a distance of about seven feet, through a hatchway, to the floor beneath. As near as could be ascertained, the accident occurred about three-quarters of an hour before. He was insensible for a time after he fell; how long a period he was unable to say, and when consciousness returned, he found that he was entirely helpless, and unable to make himself heard. He complained of intense pain in his neck, a few inches below the occiput, and on examination, I found complete paralysis, and loss of sensation below the seat of injury; heart's action very slow and feeble; pulse imperceptible at the wrist; extremities cold. Without waiting to make further investigation, he was immediately taken to the house, and on careful examination, the neck was found to be dislocated, the head turned considerably to the right side, and the deformity apparently about the fifth cervical vertebra, and very marked. As soon as the diagnosis was clearly made, I grasped the head firmly with both hands, and used all the strength I could command by way of extension, at the same time gently rotating the head so as to bring it in line with the body, as the extension was continued. This procedure occupied but a moment of time, and one of the friends who was bending over the patient, assisting me in the operation, says that he distinctly heard a grating sound, when the dislocation was reduced.

The moment the dislocated vertebra was put in position, the patient could see; before this he was entirely blind. He also expressed himself as feeling much relieved by the operation; extension, however, had to be continued almost constantly, or the pain in the neck would recur with great severity.

Vigorous efforts were now made to warm the patient, and bring on reaction—stimulants and heart tonics were administered liberally, the patient swallowing fairly well, and as the pain in the neck grew worse, morphin was used hypodermically. In about six hours reaction was fully established; the pulse beating 50 per minute, strong and regular, respiration from 10 to 12. The thoracic and abdominal muscles, however, were unable to participate in the respiratory movement, so that respiration was very imperfectly performed. The patient could now speak audibly. He took sparingly of nutritious liquids, which required some care in swallowing. His mental faculties were not in the least disturbed by his unfortunate condition, and he was able to transact business as intelligently as though nothing had occurred.

The case continued in this condition for about eight days, when the pulse became gradually more frequent, and weaker, the breathing more hurried, the inability to swallow greater, prostration more marked, sleep more fitful and disturbed. It is needless, perhaps, to say that the catheter had to be used. The kidneys, after the first twenty-four hours, acted freely, and so continued while he lived. His bowels moved freely, in response to cathartics and enemas, the sixth day after the accident, and continued to move, passively, after this, up to the time of his death.

On Thursday evening, December 7, the unfavorable symptoms all became more aggravated, the heart's action was exceedingly feeble, and the patient himself was conscious that his dissolution could not be delayed much longer. He died at 1 P.M. the following day.

When I first explained to him the very serious character of the injury he said to me: "I am in your hands. Do with me and for me as you may think best."

During the entire time of his existence, after he rallied from the effects of the shock, his intellect and mind were as active and bright as when in health.

I have, in the army and elsewhere, seen many cases of injury and death from violence, but never before had a patient that possessed such a remarkable fund of fortitude and patient endurance as Mr. Allen manifested under such painful and distressing circumstances.

He was one of the bravest, best and most philosophical patients that I ever had. In addition to this, his Christian profession, faith and character were from the time of the accident to the close of his life, made more manifest and more deeply intensified, as he approached the end.

At the autopsy, held on Monday the eleventh, the following named gentlemen were present: Drs. Wagner, Hughes and Brooke, Messrs. Kirk, Ressler and McNeilly, Dr. Wagner making the examination. All of the cervical, and one or two of the dorsal vertebræ, were removed. It was found that the dislocation involved the fifth and sixth cervical vertebræ, the attachment between these bones being completely torn asunder, and at the time of the injury, the vertebræ were widely separated, but *now* in proper position. The ligamentum nuchæ was lacerated a short distance from its attachment to the spinous process of the vertebra; considerable clotted blood was found near the seat of injury in the muscular tissue, adjacent to the spinal column. On examination of the cord, the membranes were found ruptured, at a point corresponding with the injury to the spinal column. The cord at this point and below as far as examined was softened, and completely disorganized. The line of demarkation between the softened cord, at and below the injury, and its firmness and elasticity above, was very distinct and well marked, showing plainly that the vitality of the cord, below the seat of injury, was destroyed when the accident occurred.

OVARIOTOMY IN THE AGED.

Read before the Mississippi Valley Medical Association at Indianapolis.

BY RUFUS B. HALL, M.D.

PROFESSOR OF CLINICAL GYNECOLOGY, MIAMI MEDICAL COLLEGE, CINCINNATI, OHIO.

To define clearly the limits of my topic, ovariotomy in the aged, I will take the Biblical division, that of three-score and ten years, and confine my remarks to patients who are 70 or more years of age. It is generally conceded as true, that any operation upon the old does not promise as good results as the same operation would upon the young, or patients in middle life. Especially is this true, in reference to all abdominal operations necessitating a hard strain upon the system from shock, or from traumatism to vital organs during the operation, as we not infrequently have in the removal of large ovarian tumors. Most writers, in reporting their operations upon the old women, and most textbooks, would have us believe that women 70 years old and upward are exceedingly bad subjects for ovariotomy. I am strongly inclined to the opinion that this is an error, and it is for this reason that I bring the subject before you at this time. Taking all of the reported cases of ovariotomy at my disposal, performed on women of 70 or more years of age, the mortality is very low. Especially when we take into consideration the complications, which necessarily had to be overcome in making the operations, with the facts that a large majority of these operations were delayed operations with many adhesions, large tumors and lowered vitality. The death rate is not greater in my judgment than usually follows the same delayed class of operations on women between 40 and 50 years of age. It has been asserted by one of the most distinguished writers upon this subject, that the last word on ovariotomy has been uttered; that the mortality has been reduced to a minimum, and the technique completed. While this may be true, I am inclined to believe that there are some points in reference to operations upon the aged, on which the profession are not united. It is possible that some operations upon the aged have not reached us through medical literature, yet we must believe that most of them have, and if we take them as our guide in making our deductions we certainly can not regard ovariotomy upon this class of patients as dangerous an operation as is generally supposed. I have had but three ovariotomies upon women above 70 years of age. In each of the first two cases, the friends of the patients hesitated to have the operation made on account of the advanced age of the patients, just as long as it could possibly be postponed. In each of these, the operation was deferred until the patient was in extremis.

Case 1.—Mrs. K., age 70, residence South Salem, Ohio. The operation was reported in full at the Ohio Medical Society in 1887, and can be found in the Transactions of that Society for that year. She had been conscious of the existence of the tumor for more than two years. Owing to an accident which occurred some forty years before the operation, injuring the patient's hip, she had led a very sedentary life, and had been considered an invalid for more than thirty years. For three months before my first visit she was unable to leave the room. When I first saw her Aug. 26, 1886, she was sitting half reclining in a chair, which position she had been compelled to keep for more than two months. Her pulse had been frequent and feeble for many years, and at that time as she reclined in the chair the radial pulse was barely perceptible. For two months she had anasarca of the legs, which were twice their natural size below the knees and from an abrasion upon each the dropsical fluid was discharged in great quantities. It was decided to tap the cyst with the hope of relieving her. This was done and four gallons of fluid withdrawn. This relieved her urgent symptoms somewhat for a short time, but at the end of four weeks she was again unable to rest in bed and was compelled to assume a sitting posture. She now insisted upon an operation which was performed Sept. 23, 1886. The cyst was firmly adherent to the whole anterior wall of the abdomen, as well as to the omentum, which had to be divided between successive ligatures. The posterior surface of the cyst was adherent to the small intestine and three separate coils of intestine of about one foot each were removed from the cyst with great difficulty. The bladder was spread out over the front of the cyst like a great fan and was very firmly adherent and had to be dissected from it. The hemorrhage was severe and a great number of vessels were tied. The tumor and contents weighed fifty-nine pounds. Her recovery was uninterrupted and on the twenty-fourth day she was able to leave her room, and is now at the age of 77 years, enjoying good health.

Case 2.—which has also been reported in full in the *Lancet Clinic*, is Mrs. C., age seventy years and six months. Aug. 31, 1891, while alighting from a street car she slipped and fell, causing a sharp pain in the abdomen, yet she was able to walk a short distance to her home. Two days afterwards she called her family physician, Dr. George Coner of Cincinnati, who found her suffering from acute peritonitis. She gave a history of having some abdominal enlargement for the past six months. The Doctor had no difficulty in detecting the tumor which was irregular in outline. The abdomen was very much distended. I was asked to see her in consultation the same day, July 2, and had no hesitation

in saying, from the history of the case, that the cause of the peritonitis was a ruptured ovarian tumor. An immediate operation was advised and urged, but was refused by the patient and her friends. I was again asked to see her on July 13. The abdominal distension was not so marked, yet it was evident that the patient would not long survive unless she could be relieved by an operation. So little encouragement could be given at that time, if the operation was made, that the friends were yet undecided whether to have it made or not, but consented, on the evening of July 16, to have the operation, and it was made early the following morning. At that time the patient had a pulse of 138 and a temperature of 103 degeees. The exhaustion was very marked and she had the appearance of approaching dissolution. When the abdomen was open, a gallon or more of thick dark-colored fluid escaped. The cyst which was yet about the size of an adult's head was removed. One large cyst cavity which had an exceedingly thin wall had a long rent in it and was collapsed. A tumor of about the size of a cocoa-nut was removed from the opposite side. There was septic peritonitis for eighteen days before the operation was made. The cavity was thoroughly irrigated and drained. She made an uninterrupted recovery and was able to leave her bed at the end of the four weeks.

Case 3.—Mrs. H., age 72 was seen, in consultation with Dr. Willard A. Hall of Chillicothe, Ohio, May 15, 1893. The patient had been conscious of the existence of some abdominal enlargement for about six months. She had been an invalid for more than two years suffering from bronchitis, and attributed the abdominal enlargement to dropsy. She was quite surprised when informed by her physician of the presence of a tumor, and asked to have the tumor removed as soon as possible. The patient was a feeble woman, looking much older than she really was. The abdomen was well filled and the tumor had caused some discomfort from pressure for the past five or six weeks yet she had never suffered any pain from its presence. Ovariotomy was made May 23, 1893, and a multilocular cyst removed. There were no adhesions; the cyst and contents weighed thirty pounds. The patient recovered without a single bad symptom.

Ovariotomy, when made by men thoroughly trained in abdominal surgery, and in hospitals equipped for the work, shows the lowest mortality of any of the capital operations. Judging from the low mortality of the reported cases, and from the work of the best-known operators, I am constrained to believe that ovariotomy in old women, if the kidneys are healthy, is as safe as in middle life.

154 W. Eighth Street.

IMPROVED SURGICAL ENGINE AND NEW INSTRUMENTS FOR BONE OPERATIONS;

INCLUDING A SPIRAL OSTEOTOME FOR OPENING THE SKULL IN BRAIN SURGERY.

BY M. H. CRYER, M.D., DD. S.

PHILADELPHIA, PA.

I have read with great interest the paper on new devices for cutting bone, by Dr. De Vilbiss, in THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION for Dec. 30, 1893, and though not agreeing with him in all things, heartily support his advice to the profession to the effect that there should most certainly be insisted on a better mechanical education among surgeons in general.

Too often, as Dr. De Vilbiss states, the operator does not even properly understand the use of mallet and chisel, and naturally condemns a more complicated though far superior means.

Dr. De Vilbiss' paper gives me the impression, however, that he is not familiar with the most recent surgical engines and their appliances. The engine the Doctor describes is in all its essential points similar to one introduced here some ten years ago, being light and made to fasten to the operating table or stool. It is turned by a crank, and the power conducted by means of a flexible cable many times stronger than the one used in the dental engine.

There are many objections to engines of this character. They lack sufficient power, and the limited flexibility of the cable will not allow the hand-piece to be carried in all directions, or to all parts of the body. There is also more or less vibration and backlash of the instruments, when in use, which greatly interfere with delicacy of touch.

In the *Dental Cosmos* for September, 1893, there is published a paper read by me at the World's Columbian Dental Congress of Chicago, on Aug. 15, 1893, giving the essential features of a surgical engine to meet practical requirements, and the development of the instrument, from the ordinary dental engine, through various machines, to the one I had the pleasure of introducing at the surgical clinics of that Congress; and with which demonstrations were made showing its range of applicability in bone surgery, especially in that of the head.

Since that time, operations have been successfully performed with it at the Hospital of Oral Surgery by Prof. J. E. Garretson, M.D.; in the Medico-Chirurgical Hospital by Prof. E. Laplace, M.D., and at the Orthopedic Hospital, in an extensive operation for the removal of the Gasserian ganglion, by Prof. W. W. Keen, M.D., all of Philadelphia.

A machine for operations of this kind must have motive power sufficient to give its revolving instruments a high velocity, and maintain its speed under varying pressure. In my paper, alluded to, I advocated a minimum speed of from 2,000 to 3,000 revolutions of the cutting instruments per minute; later experience has convinced me that better results are to be had with a speed of not less than 4,000 to 5,000 revolutions.

The hand-piece and chuck should be light, capable of easy adjustment, and of such arrangement that the instrument used can be freely directed to any part of the body. There should be no appreciable vibration, as it interferes with tactile recognition of the character of the tissue being cut. The device for clamping instruments in the hand-piece should permit of easy and quick exchange and they must be firmly held. The hand-piece must be easily taken apart to be cleaned and sterilized. The source of the driving power is of little import, but should be under perfect control and insure steadiness and uniformity of speed.

After using various means and devices for cutting bone, the following engine (Fig. 1), embodying suggestions from the writer, was made by the S. S. White Dental Manufacturing Co., Philadelphia, Pa.

The general appearance is similar to what is known as the cord dental engine, but it includes some distinct special features; it is many times stronger and the mechanism is of the highest grade. It will be seen that the engine is driven by hand. The crank, turning a set of gear wheels, made so true that they run noiselessly. This turns the driving wheel which is proportionately heavy, giving high speed and steadiness to the cutting instruments; near it is a pulley, T, that carries the driving cord which is likewise strong, and so arranged on the pulley wheel as to bring it into contact with more than the usual amount of the periphery of the driving wheel, giving it a grip which prevents slipping without producing the defect of an extra tension on the cord. The upright shaft has an arrangement, B, for tightening the cord and in the arm at H there is an automatic appliance for maintaining the tension

of the belt while in motion. The hand-piece is large and its arrangements for carrying various instruments are very good. It has a space, P, at its point for adjustment of different guards, etc. By removing two thumb-screws, L and M, it can be taken apart to be cleaned and made thoroughly aseptic. The arrangement of the arm and the wrist gives great lati-

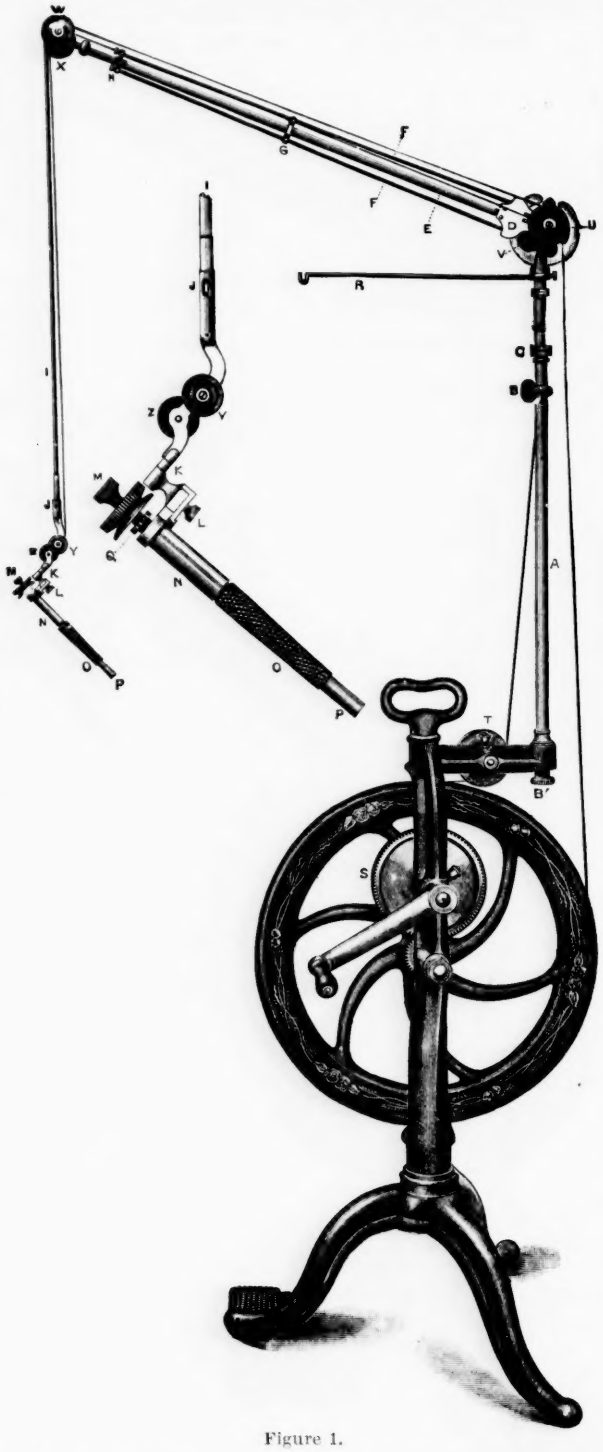


Figure 1.

tude for position and movement of the hand-piece, as great as that of the human arm and hand, while at the same time the machine may be firmly held in one position. A velocity of from 10,000 to 12,000 revolutions per minute can be attained with great steadiness and regularity of speed.

A smaller engine and hand-piece is made on the same type, and driven by the foot of the operator or that of an assistant, such an engine being all that is required for nasal or other light bone operations.

It is evident that Dr. De Vilbiss and the writer have been working toward the same end, to improve bone-cutting appliances. With an earnest desire to offer any assistance to the profession, of which I may be capable in this line, I wish to describe some of the most important instruments that have been made for the above engine.

Figs. 2 and 3 were designed about a year ago for making fenestra in the skull to furnish access to the brain in operations for removal of clots, tumors, etc., and for internal nerve resections.

The superiority of the circular saw, run at high speed by the surgical engine, over the usual method of chisel and mallet has been abundantly demonstrated. Fig. 2 represents the end of the hand-piece with circular saw one inch in diameter and one-sixteenth of an inch in thickness, inserted for use. The guard, F, is attached to the end of the hand-piece; the thumb-screws, B, are used to adjust and fix the guard to define the depth the saw is to cut.

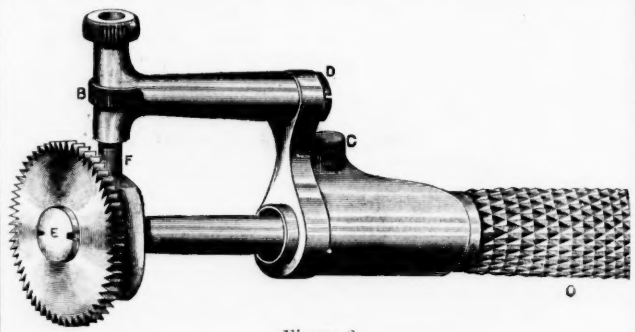


Figure 2.

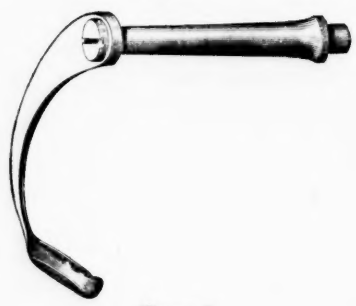


Figure 3.

Fig. 3 is a guard that can be used with the saw of Fig. 2 instead of guard F. The foot-piece of the guard, in which there is a slight groove, passes below the saw; the curved shaft is thinner than the saw and passes up back of it and is attached to the point of the hand-piece through the shank at D, Fig. 2. The object of this guard when in position is for it to be passed through a small, previously made opening in the skull, then by putting the saw in motion the guard and saw are carried in the direction in which the brain case is to be cut. The guard pushes the dura from the boneand passes between the saw and the membrane. The objection to this device is that none but nearly straight cuts can be made.

The first of these appliances, Fig. 2, was used successfully last October by the writer, in making the initial bone section in an extensive operation performed at the Orthopedic Hospital by Prof. W. W. Keen, for the removal of the Gasserian ganglion.

The advantage gained by the use of this saw was the rapidity with which it cut, yet perfection seemed lacking, in that the mallet and chisel had to be called into requisition for supplementary work. This case, as well as a series of carefully conducted experiments upon the cadaver shows, however, that all danger of wounding the dura is not absolutely eliminated, even by the use of the guard which is attached to the mandrel of the saw to prevent the penetration of its cutting edge beyond a previously determined depth. The danger exists in the fact that brain-cases vary in thickness, and even the individual skull is thicker in some parts than others, thus making it impossible to accurately fix the guard.

A number of experiments have been carried out by the writer, with the hope of overcoming the danger of wounding the membranes, either with the saw or in the final cutting of the bone with the chisel and mallet, the latter adding materially to the shock, and not being free from the possibility of puncturing the brain.

The following instrument, "Spiral Osteotome," was described by the writer in the *Dental Cosmos* for January, 1890, and the *Medical News*, Jan. 6, 1894, substantially as follows:

After many trials of variously constructed burs, the following arrangement was hit upon, which seems to completely fulfill the requirements. The cutting portion of this "spiral osteotome," (Fig. 4) is one-half inch in length, one-eighth inch in diameter, and tapers slightly from base to point. By three spiral grooves, each having a turn of 120 degrees through the half inch from base to point of the cutting end, it is divided into three blades, giving it an appearance somewhat similar to a twist-drill. The effectiveness of these blades is enormously increased by a spiral screw, forty-four threads to the inch, cut around the blades of the instrument. This device necessarily adds twenty-two teeth to the edge of each cutting blade. The individual teeth are so arranged with reference to one another that the cut made by one tooth is overlapped by that of the one next following, while the spiral arrangement of these teeth, in connection with the three spiral grooves, makes the bur perfectly self-cleaning, the *debris* being rapidly thrown out backward by the revolution of the instrument. The point of the bur when in use is guarded by a rounded button-like attachment, connected with the nose of the hand piece by means of a shank and collar, as shown in Fig. 5 at A. The free end of the bur is dowelled into a seat in the guard, B, in which it revolves, which steadies the whole arrangement when in use, gives an added rigidity to the bur-shank, and holds the bur and guard in permanent relationship to each other.

Figure 4.
Twice the size of cutting-tool.

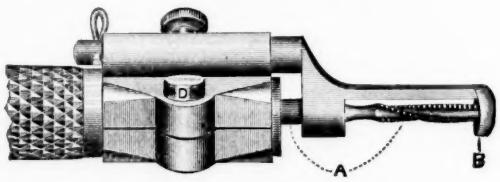


Figure 5.
Full size of instrument.

It will be seen that the principle involved in the instrument described is simply that of a saw arranged to cut in a line with the axis of the shaft of the bur, and not at right angles to it, as in the circular saw. Such an arrangement allows of cutting in any direction and upon curved lines. This feature is especially valuable in resections of the brain-case, inasmuch as fenestra of any desired shape or size may be speedily made

In operating for the removal of a portion of the brain-case with the spiral bur after division of the soft tissues by the scalpel, a small opening is first made with a trephine, Fig. 6, mounted in the engine handpiece; this trephine has a diameter of five-sixteenths of an inch, and is passed completely through both tables of the skull, and the button of bone removed. There is no danger of injuring the dura with the trephine if it be carefully used. The opening thus made affords a means of entrance for the cutting bur, which, with its protecting guard, is next inserted in the opening, and the section made along the lines previously determined, by running the engine at high speed and forcing the bit laterally in the direction desired.

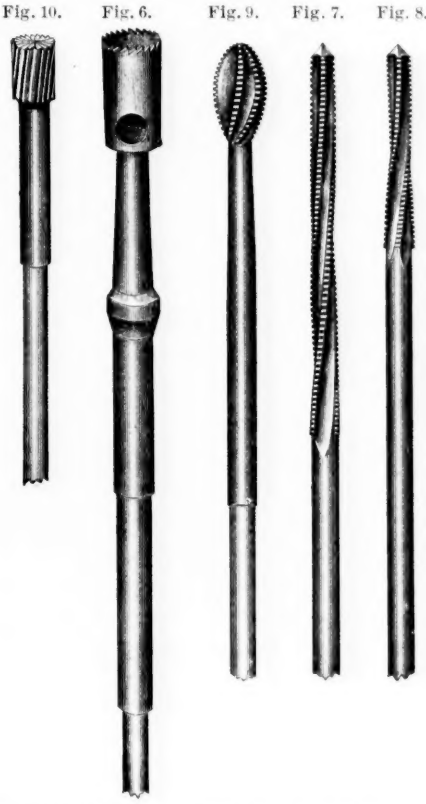
The button like guard at the point of the bur absolutely prevents injury to the dura, which is pressed or dissected away by it from its attachment in the line of the cut as the instrument progresses.

The detailed description here given is of an instrument that I have called a "spiral osteotome," and is designed for the special operation of opening the brain-case. Simple modifications of this instrument have been made for other operations upon bone. The same instrument as Fig. 4 with a rounded end and longer shaft, made in pairs of right and left, are excellent for removing diseased or nodulated bones from the nasal cavities. The great advantage of these side cutting instruments, especially in the nose, is that they do away with the to-and-fro movement of the saw and obviate the danger of cutting or injuring other tissue. The to-and-fro movement of the saw, driven by an engine through the connection of a pitman wheel and bar, was used by the writer some twelve years ago, but this character of a saw was not found to be satisfactory.

Figs. 7 and 8 are for resection of long bones, as of the femur in any part of its length, or even at the head while in the acetabulum. This form of instrument makes very fine drills. Burs of various sizes and shapes (Fig. 9) are made by increasing the number of spiral tooth blades in definite relation to the size of the cutting instrument required.

Fig. 10 represents a bur five-sixteenths of an inch in diameter. The cutting end is somewhat convex, and sides parallel, which prevents the instrument from binding. Smaller instruments of the same character are also used in mastoid trephining; when the opening made is not wide enough it can be enlarged by the side cutting portion of bur.

In the last paragraph of Dr. De Vilbiss' paper he says: "It is true, in making the mastoid operation the use of the drill has been condemned, but the



reason for it is this; that until now there has been no convenient way of running with speed a drill large enough, with precision as to depth, to make a sufficient opening." Prof. Garretson has used an engine before a large class in his clinics, and in his private practice, for at least fourteen years, which drives various sized drills and side-cutting burs up to the size of three-eighths of an inch in diameter, at the rate of from 8,000 to 10,000 revolutions per minute. Guards have also been used with some of the drills, but they are usually in the way. A few weeks ago the writer removed a diseased portion of the mastoid process of the temporal bone down to the membranes of the brain, depending only on tactile recognition. For opening into the frontal, maxillary, or any other sinuses, or, in case of osteomyelitis, guards would be very much in the way, and as no two bones are of the same thickness the guards are practically useless.

The secure fixation of the cutting instrument in the hand-piece is an obvious necessity, as any failure in this respect during an operation might possibly lead to disastrous results. In the improved surgical engine here described, the instruments pass completely through the hand-piece and are fastened in by the thumb-screw, Fig. 1 M. These chucks are made of two kinds, one to hold instruments of the size of the ordinary dental burs; the other, and the more important one for surgical work, holds instruments that are many times larger and longer in the shafts, and are held absolutely secure.

To accomplish the drilling or trephining through bones into cavities, without injuring the tissue below, the hand-piece of the engine must be free from all vibration or interference of any kind. Then the educated hand of the surgeon will distinguish the depth and character of tissue being cut.

The writer would discard all mechanical guards for drills, burs and trephines, and would indorse the following from Dr. De Vilbiss: "The surgeon must educate his hands how to execute that which he theoretically knows."

A number of the *Medical News* (April 23, 1893,) contains an article upon

"Closure of the Ear by Growths of Bone; Removal of Bony Obstructions; Cure of Otorrhea and of Deafness. Three Cases; Description of Operations, Instruments, etc." By Robert Barclay, A.M., M.D., St. Louis, Mo., in which, speaking of past and present methods for the surgical treatment of these cases, he says:

"To those afflicted with the disease in former days, grave must have been the prognosis declared, to compel, as it did, their submission to the intense and prolonged agony inevitable upon the surgical operation before the days of anesthesia and perfected mechanical arts.

"Consider, on the other hand, the manifold advantages of our modern method of dealing with such aural bony growths by means of the dental engine. In operating with this instrument, the hand-piece may be held and guided like a pen, skill and attention being addressed principally to directing the reamer or bur, inasmuch as this operates with but slight manual pressure, mainly by the rapid revolution of its cutting edges. A little experience in the use of the dental engine and drill is sufficient to educate the operator in determining by touch and resistance the nature or condition of the tissue upon which the tool is operating, thereby affording another and more reliable indication as to the desirability of extending or terminating the process of operation at that particular spot. With this there is a minimum of risk of injury to contiguous or neighboring healthy parts through direct or through transmitted force. In careful hands this instrument can not slip away or plunge. No concussion, no cracking, no roughness, no projecting spiculæ, no splinters of bone,—only a smooth, clean cut surface marks its trail."

THE DIAGNOSTIC VALUE OF EHRLICH'S DIAZO REACTION.

Inaugural Thesis read before the Minnesota Academy, Nov. 1, 1893.

BY CHAS. L. GREENE, M. D.

PROFESSOR OF SURGICAL ANATOMY IN THE UNIVERSITY OF MINNESOTA.

More than ten years have elapsed since Ehrlich announced the discovery of an urinary test which should determine the presence or absence of typhoid fever; yet up to this time the members of the medical profession of our country have not determined its proper place in our diagnostic armamentarium.

The importance of such a discovery, if genuine, makes this apathy seem at first glance almost inexplicable, but reference to the literature upon the subject offers a ready and sufficient explanation. It would seem a very easy matter for scientific observers to arrive at harmonious results in the performance of a simple experiment terminating in a definite color reaction, yet we find in the published accounts of these observations, statements so contradictory and confusing as to lead one to believe that scientific accuracy in this department at least is little better than a name. Carelessness in diagnosis and utter disregard for the instructions of Ehrlich, as to the manner of making a test, account in a considerable measure for the discrepancies of statement, but it is perhaps the failure to insist upon the appearance of the proper color in the reaction which has caused most of the error.

The discovery of this reaction was in a measure accidental, Ehrlich being engaged at the time (1882) in an effort to isolate some of the unknown aromatic bodies of the urine by the use of the diazo compounds, substances so named by Peter Griess, their discoverer, who obtained them by treating amido derivatives of the hydro-carbons with nitrous acid in a nascent state. These bodies, extremely unstable, enter readily into combination with a large number of substances. The chemical chosen by Ehrlich as being cheap and easily obtained was sulfanilic acid, which when treated with nitrous acid in a nascent state, forms in solution diazo-benzine sulphonic acid. The following are the formulæ at present recommended:

Solution 1.	Hydrochloric acid C. P	50.
	Aqua distillata Q. S.	1000.
	Sulfanilic acid ad. sat.	
Solution 2.	Solution sodium nitrite	½%
Test solution.	Solution No. 1	40
	Solution No. 2	1.

Mix.

This last solution should be freshly made each day. Solution No. 2 should be kept in a black bottle and in a cool place, being renewed at intervals of thirty days.

Ehrlich's original instructions have been modified somewhat and he now directs that, before testing, the urine should be diluted with absolute alcohol in the proportion of one volume of the former to five or six of the latter, then filtering and adding test solution to filtrate, lastly adding an excess of ammonia. For delicacy and greater rapidity of work, however, the method of Chas. E. Simon is much to be preferred, and has been used in most of the tests described in this paper. It is applied in the following manner:

Equal parts of the test solution and urine are thoroughly mixed in a small test-tube, then 1 or 2 c.c. of ammonia water are allowed to flow gently from a pipette down the sides of the tube so as to overlay the mixture. If the reaction be present, there appears at the zone of contact a beautiful colored band, and this color is apparently the rock upon which most observers have split. It is best described as carmine or blood red. Please note, particularly, that it is not yellow nor yet orange, nor should there in the opinion of the writer, be any material admixture of these colors, for were this rule adhered to order would arise from chaos, and we would not find an observer like Rutimeyer stating that the reaction is almost invariably present in a certain disease, and wrecking his observation by adding that the characteristic reaction in such cases is a yellow color. Another possible source of error arises in the presence of an excess of phosphates, the addition of a few drops of acetic acid often bringing out strongly a color reaction previously indistinct.

As to the diseases in which the reaction occurs, one can only say that the reports of the most careful and scientific observers including Ehrlich, himself, show the reaction to be present in typhoid, pulmonary tuberculosis and the exanthemata, while other observers have reported it as being present in practically every disease known to the medical profession. In view of these discrepancies of statement, therefore, this paper will be based absolutely upon the writer's own experience; as shown by the accompanying list of cases in which careful and painstaking tests were made with the sole object of ascertain-

ing the value or worthlessness of the reaction in diagnosis. The urines of about two hundred and fifty cases have been carefully examined with fairly uniform results as the following tables will demonstrate. Care has been taken to use only *fresh specimens* and a fresh and reliable test solution exactly measured. No oranges or yellows have been recognized, a well-defined carmine or blood red band being insisted upon in all cases. So, also, doubtful diagnoses have been excluded. What, then, is the result and what the inference to be drawn? We may properly dwell

TABLE I.

	Cases.	Plus.	Minus.
Albuminuria—Pregnancy	6	0	6
" Chronic nephritis	12	0	12
" Cystitis	2	0	2
" Urethritis	5	0	5
" Oxaluria and lithemia	6	0	6
Pleurisy	2	0	2
Tetanus (fatal)	1	0	1
Malarial intermittent	1	0	1
Acute rheumatism	4	0	4
Pyemic abscess of lung	1	0	1
Lobar pneumonia	2	2	0
Broncho pneumonia	1	0	1
Tuberculosis pulmonary	25	23	2
" of prostate	2	0	2
" of hip joint	2	2	0
Necrosis of long bones	2	0	2
Measles	1	1	0
Rotheln	1	0	1
Scarletina	3	3	0
Erysipelas	2	2	0
Syphilis—second stage	6	4	2
" —third stage	5	1	4
Septicemia puerperal	2	2	0
Chronic rheumatism	4	1	3
Chronic constipation	6	0	6
Alcoholic neuritis	2	0	2
Hysteria	4	0	4
Epilepsy	2	0	2
Leg ulcers (varicose)	4	0	4
Fractures long bones	5	0	5
" skull	2	0	2
Burns, severe	2	0	2
Gunshot wounds aseptic	2	0	2
Morphin poisoning	1	0	1
Sciatica	2	0	2
Cirrhosis	2	0	2
Simple enteritis	3	0	3
Angio neurotic edema	2	0	2
Vulvitis and vaginitis	2	0	2
Orchitis gonorrheal	1	0	1
Valvular heart disease	6	0	6
Quinsy	2	0	2
Typhoid, first to middle third week	19	18	1
Normal urines	20	0	20
	187		

for the most part upon its relation to typhoid fever, and I wish to emphasize the fact that in nineteen early cases diagnosed by general symptoms as typhoid the reaction is found to be present in eighteen. In the one case remaining, the diagnosis was changed to malarial fever. In eighteen out of nineteen cases, therefore, the reaction was present, and allowing for possible error in diagnosis we may fairly claim that Ehrlich's reaction is uniformly present in this disease. It was found as early as the fourth day and as late as the end of the third week. Its intensity apparently keeps pace with the virulence of the infection and it will probably be found to reappear in true relapse. It is quite possible, therefore, that its intensity and persistence may give a hint as to our prognosis. You will note, also, that in twenty-five cases of pulmonary phthisis it was present in twenty-three.[1] It must be stated, however, that these cases were for the greater part advanced in the third stage. In such advanced cases, therefore, the reaction may be said to be constant; the two cases of hip joint disease were far advanced and there was considerable septic intoxication. It may easily be that the same element of sepsis was acting in the cases of advanced phthisis before referred to. In two well marked cases of tuberculosis of the prostate in which diagnosis was confirmed by the demonstration of bacilli in the urine the reaction was absent. The relation of the reaction to tubercular diseases must be predicated upon a far more careful and extended series of diagnoses and examinations than are available at the present time. Proceeding to the exanthematous diseases we note its constant presence; also its occasional presence in tertiary syphilis and its still greater frequency in the secondary stage. Here, in fact, an almost typical reaction was obtained in every case examined, though only four of the six were admitted to the table as being absolutely above suspicion.

It is interesting to note the presence of the reaction or one indistinguishable from it in diabetes mellitus, due to action of the acid upon glucose.

Its absence in a well marked and fatal case of tetanus was somewhat surprising and the fact that the cases of malaria, acute rheumatism, and the simple diarrheas failed to show it is certainly of diagnostic importance. Septicemia does, and lobar pneumonia may give, the reaction.

In view of these facts, let us discuss at this time more particularly the value of this test in the diagnosis of typhoid; that disease of which Jaccoud has spoken as "being to-day one of the most difficult of medical problems." We seem really to have made but little advance since the day of Louis so far as our means of diagnosis are concerned. Consider, for a moment, a few of the prominent symptoms which we now hold to be diagnostic; first, the prodromal symptoms; often difficult to elicit, inconstant and indefinite to the last degree; then that "characteristic" temperature chart, so beautifully demonstrated in the text-books, but of which a typical case will rank almost as a curiosity in the wards of our Northwestern Hospitals; or, take the abdominal tenderness and gurgling symptoms which may exist in any diarrheal affection; the enlarged spleen, not always so easy to demonstrate as the text-books would lead us to imagine in a disease so frequently associated with tympanites and, moreover, common to malaria, tuberculosis, pyemia and typhoid; a rash which does not appear until the eighth or ninth day, somewhat variable and elusive in its manifestations and lacking in 20 per cent. of our cases. Even the characteristic bacillus fails to give us that positive aid in diagnosis which we might reasonably expect, for even Sternberg admits that none but bacteriologic experts can, even with reasonable certainty differentiate it from its compatriots, either by the simpler method of staining and direct microscopic examination or by more elaborate culture methods. Indeed, no better evidence of the paucity of our diagnostic resources could be produced than is to be found in recently published articles in our medical journals upon this topic. Montifusco considers a local rise of temperature in the region of the right iliac fossa, spleen and kidneys to be of the utmost diagnostic importance, Filipovitch and Skib-

[1] See supplementary remarks.

nevski have noted as peculiar to this disease a certain induration and yellowish tint on the exposed plantar and palmar surfaces, while Baruch believes that diagnosis may be predicated upon the resistance of the rectal temperature to a bath of 75° F. continued for fifteen minutes with friction. Probably almost any medical man reading these articles would be inclined to put these statements to the test when occasion might offer, yet it is unlikely that they have a fractional part of the importance of the diazo reaction. Are we then so well equipped as to rightly throw aside a means of diagnosis which seems to be the equal of any we now possess in positive value, and superior as a practical negative sign? For while the writer would not claim that Ehrlich's reaction is pathognomonic of typhoid, he nevertheless feels safe in saying that any case in which this does not appear should not be called typhoid, and if further experimentation shall continue to show that the reaction is absent in malarial fever, simple febrile diarrheas and acute rheumatism, its claim as a valuable aid to differential diagnosis will be established.

Please note, also, that the reaction may precede the roseola by full five or even seven days, a point of the utmost importance to patient and physician.

One is prepared to hear the reaction denounced as valueless because it is found in a considerable number of diseases, but such a criticism is both weak and unfair, for no other antemortem sign of typhoid fever is peculiar to it alone, and we should not deny the value of the diazo because it in a measure follows this general rule of our symptomatology. No other sign is more definite and constant. Shall we deny the importance of albuminuria as a symptom, because it is incident to a variety of pathologic conditions? We should not insist that our reaction must differentiate typhoid fever from all other diseases, before admitting its value as a clinical sign; on the contrary we ought to give it due credit for the large amount of assistance which it bids fair to give us.

We may fairly hope that it will be of great service in differentiating typhoid from malaria, acute rheumatism and the simple fevers with diarrhea. Opportunities for testing the reaction of appendicitis have not yet presented themselves to the writer.

To sum up, then, this much neglected reaction gives us in typhoid fever much positive, a considerable amount of differential, and a very large amount of negative information. It is not in accord with the high standing of our practitioners in this Section that this method is not at the present time on trial in all of our hospital wards, and without asking that you accept these views as your own, the writer would earnestly urge all present to make use of the test, feeling confident that their trouble will be well and fully repaid. Materials are cheap and easily obtained, the formulæ are practical and simple, but to obtain good and uniform results the following rules should be strictly observed: 1, *Freshly passed specimens of acid urine must be used*, decomposed urine being discarded, and all alkaline or neutral urines acidified.

2. No color but red should be accepted.

3. The test solution should be freshly prepared for each day's use and measured with extreme care.

4. Sodium nitrite solution should be renewed frequently and kept from light and heat.

5. Finally, and most important of all, the exact method of procedure must be carefully followed out *in every detail* on the lines heretofore laid down. One might add, be absolutely certain of your diagnosis before you criticize the test. For it has again and again proven the error of a hasty diagnosis. The experiments of which this paper is the record have but the limited value of a preliminary report; they should be extended and multiplied, and the results carefully analyzed, before we may pass finally upon the merits of the test. The writer can only speak for himself, when he states positively that the test has been and is now of very considerable value to him, personally, in his clinical work, and having previously been a skeptic it is possibly with the added fervor of the proselyte that he urges its more general adoption.

SUPPLEMENTARY REPORT OF THE RESULTS OBTAINED BY A MODIFICATION OF EHRLICH'S METHOD OF TESTING THE URINE IN TYPHOID FEVER.

The reading of my original paper having been postponed, I have occupied the intervening time in making new tests for the further extension of my tables, and in the hope of more sharply defining the limits of occurrence of the diazo reaction. For some time I have felt convinced that such a change in the test solutions might be made as would eliminate some of the doubtful reactions, and possibly show that in tuberculosis, etc., there were either qualitative or quantitative differences acting to obscure the results. Using a concentrated test solution with the sodium nitrite in excess, I found the reaction or one closely resembling it even in normal urine, and acting upon this hint the process was reversed, and instead of the 1:40 solution prescribed by the authorities I have used a solution containing but one part No. 2 to 100 parts No. 1, and find that by so doing the oranges and mixed reds and yellows mostly disappear. Pulmonary tuberculosis and pneumonia fail to yield a reaction, while it remains distinct and perfectly defined in septicemia, typhoid and advanced malignant disease. The following table will show the result of the modified test:

TABLE II.

	Cases.	Plus.	Minus.
Typhoid	10	10	0
Septicemia	3	0	3
Varicella	1	0	1
Typhoid relapse	2	2	0
Pseudo-relapse	1	0	1
Scarlatina	1	0	1
Phthisis pulmonalis	12	0	12
Lobar pneumonia	3	0	3
Febrile diarrheas	2	0	2
Pleurisy	2	0	2
Syphilis—second stage	3	0	3
" —third stage	2	0	2
Malarial fever	2	0	2
Carcinoma advanced	2	2	0
Chronic rheumatism	3	0	3
Simple anemia	1	0	1
Heart disease	4	0	4
Chronic nephritis	6	0	6
Tuberculosis of hip and elbow joints	1	0	1
Arterio sclerosis	2	0	2
Cirrhosis of liver	2	0	2
Oxaluria	3	0	3
Gastric ulcer	1	0	1
Acute bronchitis	2	0	2
Chronic constipation	1	0	1
	75		

You will note that by this method of testing

typhoid fever, cancer and septicemia alone give the reaction. It is probable that the severer exanthemata would also show it. In typhoid it was found present as early as the third day, and persists as long as fever is present. *It positively does not occur in lobar pneumonia or pulmonary tuberculosis*, if this method of testing be used, though present in both diseases when the 1:40 solution is used. This modification would seem therefore to add considerably to the value of the test, both in direct and differential diagnosis, and still more strongly suggests its utility in general diagnosis. You will note, for example, that it does not occur in chickenpox, while it has been invariably reported as present in variola. That it is absent in rotheln though present in measles, while its appearance in cases of malignant disease, if shown to be constant, would be of the greatest value in the diagnosis of obscure abdominal affections and the differentiation of malignant and benign growths. In typhoid relapse, as in the original disease, I have found it present as early as the third day of temperature. Before closing, I desire to emphasize the importance of verifying the color test by shaking and carefully noting the appearance of the surface foam. If the reaction be present, this foam invariably shows a delicate pink or rose-red coloration, while in doubtful or negative cases the foam is yellow. This little expedient greatly simplifies the test. I sincerely trust that further reports upon this matter will be forthcoming, and am positive that the verdict founded upon actual experience will be favorable to the test.

INEBRIETY.

BY W. P. HOWLE, M.D.
ORAN, MO.

I am just through reading a paper by R. M. Wiggington, M.D., of Waukesha, Wis. He refers to his Missouri friend, meaning me, because I do not believe with him that "the drink habit," is a *disease*. His reasons, as given in his paper, for believing that it is a disease are many, well stated and seemingly true, but he, like other men who have a theory they wish others to believe, presents only one side of the question. They never mention facts that would seem to contradict their views. I will now state a few reasons why I do not believe in this modern theory. I say modern, because, as far as I know, this theory is of recent date. Drunkenness or inebriety has for centuries been looked upon as a *vice* and all governments of which I have any knowledge have laws to prohibit inebriety and penalties for violation of said laws. Our Government is not an exception, but I do not believe there is any law to punish men for becoming ill. Every citizen has a legal right to be sick. The law-making power makes quite a distinction between the two conditions. It follows from this, that the ablest men in our Government do not consider inebriety a disease. Legislators for centuries have said by their acts: Inebriety is a vice and not a disease (as some of the learned medical men would have us believe). The Doctor says: "It is generally conceded by the scientific world that inebriety is a disease." Why, then, does the scientific world not convince the law-making power that such is true?

The senators and congressmen of our Government are thought to be our ablest men. You do not hear them saying anything about inebriety being a disease. If you were to approach one of them who goes on an occasional jag and tell him he was diseased and not fit to be a senator, but ought to be sent to some asylum for inebriates, you would see how much he believed in this modern fad. Every believer in this theory knows of one or more of his neighbors who have gone to every extreme which drunkenness will induce, and who have reformed, lived on in the same neighborhood, become good citizens, had the confidence of all, who never took a dose of medicine for this so-called disease. Will any of you please point out just what ailed these men? You assert that the drunkard is diseased in every cell and has no moral power. If so, how did these, your neighbors, reform? Can a man who has no moral power reform; and if these reformed men are diseased, why do they not show some manifestation of disease? They eat, drink, sleep and work like well men. They have normal temperature, circulation and digestion; are men mentally able to practice medicine, law, or preach the Gospel; can do any kind of business they are called upon to do. You say they are diseased. How do you know? I venture the assertion (I believe it is a fact) that more men have been reformed by moral suasion than in any other way. If moral suasion will cure disease, and especially one so serious as my opponents make inebriety to be, why will it not have at least some effect in other maladies? I believe large quantities of alcohol taken into the stomach would produce gastritis. Now will any one assert that moral suasion would have any effect on gastritis? So I might enumerate the whole catalogue of diseases produced by excessive drinking, and moral suasion would never be thought of as a remedy. Yet by moral suasion, alone, the worst drunkards on record have been reformed and restored to usefulness. No man can explain this process from a standpoint of *disease* and *cure*.

Suppose some of my opponents should claim that they were having wonderful success in curing pneumonia, and say they were using only *moral suasion*. Would they not be called cranks? There is as much sense displayed in such a claim as there is in saying inebriety is a *disease*, and moral suasion will cure more cases than any other one remedy. No one will pretend to deny that the drink habit is being cured every day by moral suasion—also by religious influence. Now, this being true, if the drink habit is a disease and religious influence will cure it, it follows that religious influence has power to change diseased cells into healthy ones, and having this power would be a sovereign remedy in disease, but the medical profession have "sat down" very hard on the christian science frauds. They hoot at *faith cures*, but if their theory is true, the faith cure gentlemen ought to have due credit for their work Christian science has a right to a top seat if inebriety is a disease. Why? Because religion cures more drunkards than medicine cures. But the facts in the case are these: The drink habit is *not* a disease and the so-called christian science is a fraud, and this argument knocks the *disease* side of the question into a "cocked hat." Now for a few reasons why this theory won't go: It is contrary to the teachings of the Bible and the justice of God. The Bible says: "No drunkard shall inherit the Kingdom of Heaven." Would the God of Heaven shut the gates against sick people? If God had not considered drunkenness a crime He would never have issued a decree against it. Ask

any man who gets drunk whether he considers it right or wrong? Ask him what the Bible says about it? Let every reader ask himself what he thinks about it, and my word for it the universal verdict will be: Drunkenness is a vice, a sin against God's commandments and against one's own present and future happiness. If the Bible teaches truly, it is evident that inebriety is a sin and not a disease.

A word or two as to heredity and I am done. A man may inherit an *appetite* for whisky, but craving an article is not possession of it. A man may want many things that he will never get. A man must drink to become a drunkard. A born drunkard is a misnomer. A man must eat to become a glutton. He must smoke to become a cigarette fiend. My opponents have the cart before the horse. They say the man drinks because he is sick. I say he is sick after he drinks, not before. They say illness caused the drinking. I say the drinking caused the illness. I do not expect to convince them of their error, but write only in defense of what I believe.

A CASE OF DIPHTHEROID CHANCRE ON THE POSTERIOR WALL OF THE PHARYNX.

BY JAMES HARVEY RAYMOND, M.D.

CHICAGO.

Instructor in Surgical Pathology in Rush Medical College; Assistant to the Clinic Department, Nose, Throat and Chest, Rush Medical College; Attending Physician at Central Free Dispensary Department, Nose, Throat and Chest.

A chancre is the primary lesion of syphilis, which makes its appearance at the seat of inoculation, usually from twenty-one to thirty days after contact with the essential cause of the disease, which at the present time is not definitely known.

The physician who has had the opportunity of observing patients suffering with the disease in its various phases, skeptical though he may be, must yield to the preponderance of clinical evidence, which favors the consideration of this being a disease caused by microörganisms or their ptomaines.

This theory is strongly supported by the fact that certain phases of the disease or its deleterious effects upon the tissues of the body, produce pathologic conditions analogous in character to other well-known diseases of microbic origin, viz.: yaws, glanders and other granulomata. While the clouds of uncertainty are hanging over us regarding this disease, the search light is still in the hands of modern pathologists who by patient and diligent scientific research, it is hoped, will soon point out and prove positively its true cause.

Reviewing the etiology of syphilis, we find foremost among those who have studied by experimental research and have expounded the theory of microbic infection, the names of Lustgarten, Doutrelepont and Schutz; Matterstock, Markuse, Andromico, Zeissl, Marcus, Disse and Taguchi; Eve and Klebs. (Senn, Surgical Bacteriology, 2d edition, pp. 243–246.)

But none of the above pathologists have satisfactorily shown the constancy of the microbe they claim as the cause, and as no uniform results have been obtained we are still in the dark, and the true etiology remains to be shown by future investigation.

A chancre may exist on any part of the cutaneous or mucous surface of the body;—the most common seat is on the penis, glans, prepuce or urethra. It is also found about the anus, lips, mouth, tongue, palatine arches, tonsils, cheek, nipples, etc. It is the *extra genital* class that is the subject of this article and that is by far of greater interest to us from a diagnostic point of view, as its somewhat varying and peculiar characteristics are too apt to be overlooked by the inexperienced or busy practitioner of medicine, or taken for a more simple ailment, the patient thereby being allowed to harbor an affection that sooner or later will claim him as a victim of its ravages and perchance cause like suffering in many others.

The macroscopic appearance and consistency of a chancre vary according to its anatomic situation; it makes its initial appearance in the form of a small papule, hard and painless, gradually enlarging and becoming more indurated, especially about the edges, until finally it presents the appearance of a hard, circumscribed mass, the center of which is somewhat depressed.

When it is found upon mucous surfaces, e. g., in the mouth, it appears as a patch, white or yellowish gray in color, circumscribed, and elevated above

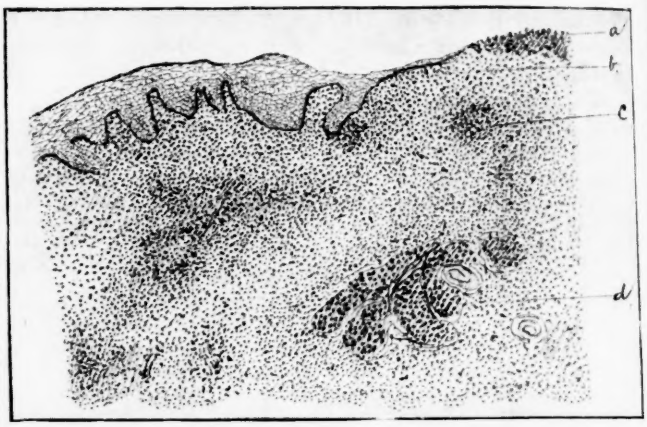


Fig. 1.—Perpendicular section through edge of hard chancre of prepuce. Hamilton.
a. Ulcerative surface. *b*. Epithelial edge. *c*. Dense small cell infiltration. *d*. Lymphatics encompassing an artery, filled with small round cells.

the surrounding tissue and presenting a variable degree of induration about its edges and base. When the tissue about the center becomes favorable for the growth of pus microbes, the center becomes cup-shaped, due to the ulcerative process extending downward, and contains the histologic elements of pus.

Microscopic examination of chancre reveals a dense, small, round cell infiltration of the tissue, which by pressure from beneath towards a point of low resistance, causes the upper stratum to become greatly attenuated at the center of the lesion, the atrium of infection. About the edges the papillæ of the derma are seen to be hypertrophied, the tunica adventitia of the blood vessels is sclerosed and the lymph spaces become filled with round cells. As the process of the proliferation continues, the cells, owing to the weakened condition of the tissues at the center, work their way towards the surface and very often burst through, causing a granular excoriated or abraded appearance.

The amount of induration about the edges is due to the degree of hypertrophy of the tissue and sclerosis of the tunica adventitia of the vessels in the

papillæ, together with the concomitant round cell infiltration of the adjacent lymphatics and peri-vascular tissue. Various kinds of bacteria are found, and can be seen in great abundance, and if ulceration takes place, it is caused by the specific action of pyogenic microörganisms upon the histologic elements of the lesion, which furnish a favorable medium for their growth and multiplication.

A rare variety of the initial lesion of syphilis is the diphtheroid chancre of Dr. Morrow, in which a large portion of the glans penis is covered with a glistening grayish-white coating of a leathery consistency, simulating in all its characters a diphtheritic exudation. He states that in one case in which a patient confessed the *rôle fellatrix*, the appearance of the throat suggested a malignant diphtheria, though lacking adherent membrane. Clerc, also, has observed this form of lesion and describes it: "As a kind of false membrane presenting some resemblance to the diphtheritic patches, which characterize certain forms of syphilitic symptoms, affecting mucous membranes, but usually occupies only the center of the chancre, its edges shading off into a reddish circumference."

Cornil found in the transparent fluid on the surface of a chancre, pus corpuscles, fine albuminous or fatty granules, very small spores separate or forming rows, rod-shaped bacteria and corneous cells of the rete mucosum undergoing destruction.

The frequency with which the extra genital chancre is found in the throat, in the practice of the average physician, may be estimated from the reports made during the past few years by the following syphilologists and laryngologists. The late Sir Morell McKenzie in all his extensive experience met with but seven cases of chancre of the pharynx, and states: "The rarity of chancre of the pharynx may be estimated from the fact that of 673 examples of chancre in all situations, not one was found behind the anterior pillars of the fauces."

The primary lesion of syphilis, when situated in the pharynx, is most always found on one of the tonsils. Desnos attributes this to the structure of these glands, the lacunæ of which are likely to receive and retain the syphilitic virus when introduced into the throat.

Dr. Morrow, in his most excellent work on syphilology, of recent publication, says that chancre of this region is most frequently found on the tonsil, the posterior wall of the pharynx being most singularly exempt.

Diday met with eight cases of chancre of the tonsil.

Dr. L. D. Buckley of New York, (*New York Medical Record*, May 27, 1893, p. 645) states that in twenty years of practice more than two thousand cases of syphilis came under his observation, 5 per cent. of which were extra genital, fifteen of these being found on the tonsil.

Dr. Cæsar Boeck states that in Norway, chancre of the pharynx is next in frequency to the genital regions, attributing this to the customs of living, having observed one large family all using the same spoon, without cleansing. M. S. Usass showed at a recent meeting of the St. Petersburg Russian Syphilogical and Dermatological Society, a married woman, age 41, with chancre of the right tonsil and posterior pillar of the fauces. A. I. Budngoff also presented a recruit, age 24, and S. Serdnekoff relates two cases of chancre of the tonsil, with chancre of the right tonsil. Neuman saw but one case, of eighty-eight extra genital chancres; eight were of the tonsil. Columbini of Sienna, and Cartaz of Paris, have recently reported cases of chancre in a similar location. It is said that the majority of cases of extra genital chancre are found on the lips and tongue, next in frequency the tonsils. But the rarest of all as shown by the foregoing facts and reports is that of the *posterior wall of the pharynx*. It is owing to the fact of its great rarity in this location, that I am pleased to be able to report a case, and add the notes made while the patient was under close observation.

Mr. F. came to the Chicago Policlinic (Department Nose and Throat) for treatment; his age 31; unmarried. Had never had any disease, other than ailments incidental to childhood; had not been directly exposed to any infectious disease. Family history good. Three weeks previous felt a sensation of dryness and had a desire to hawk and clear the throat; was languid; appetite poor; had headache and considerable jactitation; felt slightly feverish. The following day he noticed a white patch the size of a nickel on the posterior wall of the pharynx in the median line, which seemed to be rapidly spreading and in less than twelve hours had extended down the pharynx into the naso-pharynx and on to the anterior pillars of the fauces.

This condition had continued three weeks, with no change of the above symptoms, except that the fever was higher, and deglutition painful, owing to soreness of throat. The appetite was very poor, voice hoarse and of marked nasal quality. Examination of the pharynx revealed the presence of a smooth yellowish-gray membrane, one-sixteenth of an inch in thickness, symmetrically covering the posterior wall of the pharynx, extending down about half an inch, and into the naso-pharynx one-third of an inch, and about a quarter of an inch on the anterior pillars of the fauces. It was quite adherent to the underlying tissues—the edges were elevated and the tissues about the periphery were greatly indurated and quite deeply congested. The tonsils were very slightly enlarged.

The membrane was removed with some difficulty and when detached gave rise to slight bleeding. When removed in toto, the floor upon which it had rested presented a faintly granular appearance, the edges of which were quite markedly indurated. There was no perceptible ulceration. The glands in the cervical region at the angle of the jaw on either side, were greatly enlarged. The temperature registered 101 3-5; pulse 92, regular, feeble. The appearance of the throat and clinical history, though not typical, seemed to justify at least a suspicion of diphtheria. The patient was isolated. The following treatment was instituted. Having used, with considerable success, in acute tonsillitis and pharyngitis pure guaiacol locally, it seemed rational to try it in this case, knowing its efficient antiseptic properties. I made the application at 5 P.M. on January 6, and saw the patient the following day at 1 P.M. The temperature was 98 6-10, pulse 90. Guaiacol was again applied and at 6 P.M. same day, pulse was 80, temperature 100. The patient had enjoyed a good night's rest—his headache had passed away and he felt very much improved. Guaiacol was continued five days, the temperature in the meantime ranging from 98 6-10 to 99; pulse 72 to 100.

℞ Ferri et quininæ citratis, ... grs. iii.
Ext. nucis vomicæ, ... grs. ss.

Was given before each meal and at bed time.

Guaiacol was withdrawn, after which the pain seemed more intense, pulse slightly accelerated and temperature elevated. At this time he complained of inability to swallow fluids readily on account of regurgitation into the nose. The nasal quality of voice seemed more perceptible and a well-marked paresis of the pharynx and palate had developed.

Antiseptic gargles, nasal douches and all other symptomatic treatment was promptly given, but from the start it seemed at best only palliative, there being no perceptible change either for better or worse.

The membrane was examined microscopically for the Klebes-Löffler bacillus; and cultivation experiments carried out—negative results. The case was examined by Dr. E. Fletcher Ingals who was of the opinion that it was very strongly suspicious of diphtheria. After having had the patient under close observation for two weeks, I noticed a

few personal peculiarities that to me were strange and suspicious. He was not effeminate in his appearance, but at times quite so in actions. About this time I noticed a friend, as he called him, his room-mate, whom I saw for the first time. He was very effeminate, both in appearance and actions and was the signal for arousing strong suspicions of the character of the patient. A typical papular syphiloderm shortly made its appearance over the entire body of the patient; one or two mucous patches were seen on the lips and mouth. The patient was confronted with the fact that his history was well known and asked at what time he had peculiarly exposed himself, according to the custom of a certain class of sexual perverts. He hesitated at first, but finally confessed that he had been addicted to these practices since childhood. Hydrargyri proto-iodid grs. ¼ t. i. d. was given, and in the course of ten days the subjective symptoms had completely subsided and the membrane disappeared.

Portions of the membrane had been prepared for microscopic examination in the following manner: After standing in dilute alcohol for a few days thin sections were made. When these were carefully teased with needles, they were found to be torn with considerable difficulty, and were quite elastic. A section was stained with fuchsine, mounted and examined with a high power, and was seen to consist

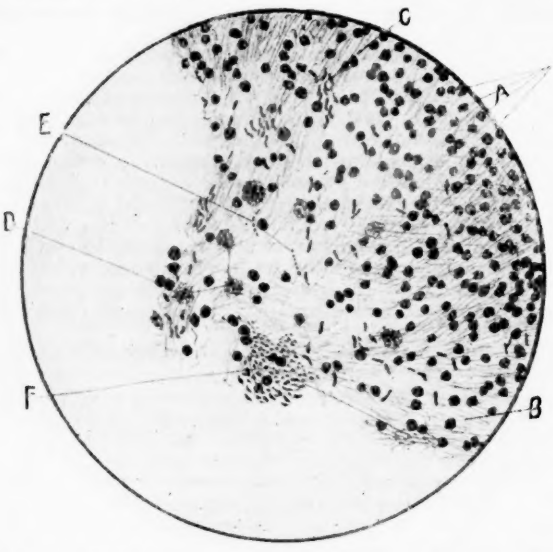


Fig. 2.—Central portion of membrane from diphtheroid chancre of posterior wall of pharynx, five weeks after its initial appearance.—J. H. Raymond.

A. Dense round cell infiltration. B. Fibrinous reticulum. C. Large rod-shaped bacteria. D. Pus corpuscles. E. Streptococci. F. Colonies of staphylococci.

of a dense felt-like mass of fibrils. Upon examination at the edges of very small fragments, obtained by teasing, the fibrils are seen to divide and sub-divide into short branching prolongations, resembling stag horns. This reticulum, fibrinous in character is no doubt fibrin. A certain amount of coagulation necrosis attends the formation of fibrinous organization wherever met, e. g. pleuritic exudate. Densely infiltrating this reticulum can be seen small round cells. A few streptococci, colonies of staphylococci, pus corpuscles and a great many rod-shaped bacteria are also present.

From the clinical facts and microscopic findings in this case, which are identical with the histologic elements of chancre as given by Cornil, Hamilton and others, and the fact that there was a prompt response to anti-syphilitic treatment, I am confident that we have been dealing from the start with a case of diphtheroid chancre of the posterior wall of the pharynx.

36 Washington Street.

REGISTRATION OF TUBERCULOSIS.

Special meeting of the College of Physicians and Surgeons of Philadelphia, held Jan. 12, 1894, the President, S. WEIR MITCHELL, M. D., in the Chair.

TO CONSIDER THE PROPOSED ACTION OF THE BOARD OF HEALTH IN REFERENCE TO THE REGISTRATION OF TUBERCULOSIS.

(*Continued from page 226*).

DR. WILLIAM OSLER, Baltimore—The question may be thus briefly stated:

1. Following a primary law of parasitism, the bacillus tuberculosis frequents chiefly that organ in its host which communicates most freely with the exterior. Just as countless thousands of ova are thrown off from the intestine of the bearer of a tapeworm, so from tuberculous lungs in a state of softening and cavity formation, countless millions of bacilli are cast out daily with the sputa.

2. The widespread diffusion of the parasite outside of the body has been demonstrated in the infectiveness of the dust and of the scrapings of the walls of rooms and wards occupied by patients with pulmonary consumption. Moreover, the greater prevalence of tuberculosis in crowded communities, the enormous mortality from the disease in prisons and institutions, and its frequent occurrence as a house malady, suggest that the conditions favoring its continuance are those which foster the growth and spread of a specific contagion.

3. In the language of parasitology the lungs constitute the chief seat of election! But, apart also from gross pulmonary lesions, the proportion of autopsies in which the bronchial glands are found tuberculous, speaks unmistakably for direct infection in the exercise of their function as dust filters.

On these grounds I believe that the registration of pulmonary tuberculosis would be beneficial—enforcing attention to those sanitary details so apt to be slighted or overlooked, and diminishing directly the danger of contagion in the community.

Infection through food is closely related also to the endemic prevalence of tuberculosis. The incidence of the disease in the mesenteric glands of infants indicates that the gastro-intestinal canal is a portal of infection only a little less wide than that of the respiratory system.

The question of tuberculosis is not, however, settled with the disinfection of the sputa of consumptive patients.

The hereditary transmission of the disease must be accepted, though an estimate of the frequency of this mode of infection must necessarily be uncertain, but for certain forms Baumgarten's theory of latency is particularly suggestive. Tuberculosis has been well called the *pébrine* of the human race. The analogy is striking, for not only as you will remember, is the parasite of the silkworm disease transmitted by direct contagion, but it also infects the eggs, which hatch and may pass through various stages of development before they are finally destroyed.

Lastly, and here is consolation, the conditions which render individuals more or less immune scarcely yield in importance to those which maintain the vitality of the tubercle bacilli in a community. So widespread is the seed that few of us escape infection, and the statistics of the Paris morgue show that in more than 50 per cent. of adults the germs not only gain an entrance, but actually effect a lodgment. As a factor in tuberculosis, the *soil*, then, has a value equal almost to that which relates to the *seed*, and in taking measures to limit the diffusion of the parasite let us not forget the importance to the possible host of combating inherited weakness, of removing acquired debility, and of maintaining the nutrition at a standard of aggressive activity.

DR. H. C. WOOD—I came here not to speak but to learn, and thus far have been quite successful. I wish to quote a letter from Dr. Billings which has been placed in my hands. He says: "I should like very much to hear the discussion, but probably should have little to say, since I am doubtful as to what the decision of the College should be on this point. I presume that there are about six thousand people in Philadelphia affected with consumption, and that a considerable proportion of these have contracted the disease in infected houses. If it were possible by a systematic notification for the Board of Health to locate a considerable number of these infected houses, what steps would it take to purify them? How would it deal with those of the poorer classes who are affected with this disease, and with their furniture, bedding and rooms? Until these questions are

answered, I do not find it possible to form a definite opinion as to whether it is worth while to put in force a compulsory system of notification. Some of the worst infected rooms are probably those occupied by the criminal class, which is especially liable to this disease. For example, the Eastern Penitentiary is certainly thus infected; but in this I presume the Board has no jurisdiction.

"It occurs to me, that if the system of notification is to be tried, it might be best to commence with a limited area of the city in which the death-rate is now high; for example, the Seventeenth or the Seventh Ward, and see what results in diminishing the proportion of cases in this area can be obtained in the course of two or three years."

This thought has occurred to me, and has not been touched upon by any of the speakers. In a case of diphtheria the Board of Health is notified. The patient dies or gets well and the room is disinfected. Suppose, however, a case of consumption; the Board of Health is notified, the house is disinfected to-day but reinfection occurs tomorrow, and the end need not come for four or five years. Unless you add the power to forcibly remove the patient and shut him up in a hospital-jail where you can confine and permanently disinfect him, I do not see how you are going to get any good results from the process of disinfection. As far as disinfection goes, unless you can prevent to-morrow's reinfection, I see little use in to-day's disinfection. If, however, the Board of Health could disinfect houses in which phthisic cases have died, something might be achieved, but the ordinary death notice is all the notification required for this.

Dr. Thomas J. Mays—In the charts which are presented to us it is to be noted that the disease is less frequent in those parts of the Fifth Ward in the neighborhood of Spruce and Pine Streets, and becomes more prevalent as we approach the poorer portion of the city. Dr. Flick's investigations extended over only a limited period of time, say twenty-five years. In that time he finds that about 25 per cent. of the houses in the Fifth Ward have become infected. I hold that if the investigations had extended over a longer period—say fifty, seventy-five, one hundred, or one hundred and twenty-five years—he would have been able to show that nearly every house in that ward was infected, according to the contagion theory, that in nearly every house in that ward a consumptive had lived and died. The same argument holds against the investigations made in New Haven. I have in my possession a book reporting similar investigations made by Dr. Riffel in two old towns along the Rhine in Germany. He investigated the death-rate from phthisis in these towns for fifty, seventy-five, one hundred, and in some cases one hundred and fifty years back. He found that in one town at least—Karlsdorf—that at some time or other nearly every house had had a case of phthisis in it. I think the same thing would hold in regard to the Fifth Ward of this city if the statistics extended back sufficiently long. But as these only cover a period of about twenty-five or thirty years this is out of the question.

Dr. Flick also refers to the diminution of the phthisis mortality in Berlin, London and Philadelphia. I think that if he had consulted the death-rate from phthisis in these cities he would have found a gradual diminution in the death-rate from consumption before the bacillus area began in 1882. I went over this subject some years ago and I found that in every large city there was a gradual diminution in the death-rate. Dr. Flick claims that the diminution in the death-rate of consumption, since 1882, in this city has been due to the disinfection of the sputum. His own figures show, however, that there occurred a larger number of deaths from this disease during the nine years following 1882 than during the nine preceding years. In what way this makes a favorable showing for the theory of disinfection I am at a loss to understand. So far as I know there has been no Board of Health regulation in this city. So far as I know there has been no Board of Health regulation in Berlin, or London, or any other city, yet their deaths are gradually diminishing in number. If the gentlemen on the other side will look backward a hundred years they will find that the very experiment which they are so anxious to try now was tested with the utmost rigor, from 1782 to about 1855, in Naples. It was decreed that every physician who neglected to report a case be fined $180; that the ceilings, walls, floors, doors, and windows of the room in which consumptives died be torn out and burned; that the bedding and furniture be also burned, and that such dwellings were not to be occupied for a year. The result was that the family with phthisis was shunned and driven to want. Houses in which consumptives died depreciated in value. The sick were neglected and left to die inhumanly away from their families and friends. Did any good result from these laws? Brehmer states: "Concerning a diminution in the rate from phthisis in Naples the medical historians of that period are ignorant." According to Uffelmann (*Berlin klin. Wochenschrift*, 1883, p. 369) Dr. de Renzi, the medical historian of Italy, states that the injury which had been inflicted on Naples by these laws was simply indescribable and denounces the Neapolitan faculty in the severest terms for participating in their introduction.

In Vol. xlv., p. 112, of the *British and Foreign Medico-Chirurgical Review* it is stated that Drs. Spatuzzi and Somma, "who have paid great attention to the mortuary returns in that city (Naples, about 1860), affirm that one-sixth or a seventh of the whole mortality is due to phthisis," and that Dr. de Renzi "marvels greatly (in 1863) that the city of Naples is fully as much liable to phthisis as either London or Paris, though the salutary condition of the climate should render it far less common."

If the death-rate from consumption was the same in Naples at the time segregation ceased, as it was in other cities in which segregation was not practiced, it is self-evident that such a measure can have no influence in diminishing the death-rate from this disease.

Dr. James Tyson—I had intended to say something this evening on this important subject, but almost every point which occurred to me has been so well treated by others that I hesitate to add anything. I admit all that has been said with regard to the contagiousness of phthisis, as determined by experiment, but as to the actual spread of consumption from one person to another as the result of association—as to the degree of contagiousness, in a word—I agree entirely with Dr. Da Costa. Admitting the correctness of Dr. Flick's studies and Cornet's observations, it still comes to this, that the physician is competent to take care of this question without the intervention of the Board of Health. Just as the surgeon is qualified to take care of the matter of antisepsis and asepsis in his management of surgical cases, so the physician is competent to take care of the disinfection utensils used by consumptives and the destruction of the germs which may be the cause of the disease, the more especially as such disinfection is acknowledged to be of the easiest kind. What Dr. Roberts has just said with regard to the situation ten years ago as to sepsis and antisepsis, could as well be alleged in favor of the position that I take—that the physician is competent to handle the difficulties. The surgeon did not call in the Board of Health to help him under like circumstances but fought the battle alone, and won it triumphantly. The question has been studied experimentally and chemically, and we know exactly where to put our fingers to check the spread of consumption as far as this is possible. We must attack the disease in two ways: 1, the contagion must be destroyed as far as possible with the means at our disposal; and 2, we must seek to change the soil on which it is known that the bacillus flourishes, by improving the general health of the individual, and thus increase his ability to repel the invader. I feel that we are entirely competent to handle this question without the aid of the Board of Health.

Dr. Abbott—I have listened with much interest to both sides of the discussion, but must confess that I am not yet quite secure in my mind as to the proper course to be pursued. I am strongly inclined to the views expressed by Dr. Billings in his letter to the President. Tuberculosis is a widespread disease with manifold expressions, and if our efforts are to be directed against it, in the full sense of the term, the problem is certainly beset with many difficulties.

In connection with the subject under discussion, the question that has constantly presented itself to me is: If the College sees proper to recommend that all houses in which the disease is located be reported to the authorities, has it any guarantee that their conditions will be improved? The object in reporting these houses is to secure complete disinfection of them, a process requiring conscientious attention to most minute details, to say nothing of a full knowledge of the requirements of the cases. If the Board of Health is in a position to assure us that these demands can and will be met, then I am in favor of the resolution. If not, then we have had a long discussion with but little result.

The ultimate object arrived at in the resolution I consider as most desirable, but will it be attained if the resolution is passed? We have abundant evidence in justification of the belief that localities occupied by persons suffering from the pulmonary form of tuberculosis, particularly, may and do become centers of infection, and it is highly desirable that such places should receive the attention that their charac-

ter demands. Much has been said here this evening concerning the prevention of the spread of this disease by disinfecting the sputum, and this point has been accentuated as if the danger ended here. The danger does not end here. Sputum collected in a receptacle may remain perfectly harmless without disinfection or any other treatment whatever. The danger lies in the sputum *not* collected, but spat upon the floor, dried, ground into dust by the feet of passers-by, and ultimately inhaled into the lungs—a condition of affairs often seen in dwellings of the poor. The predominance of pulmonary tuberculosis over other manifestations of the disease is certainly, in part, accounted for in this way. Dr. Da Costa in his remarks has said that "bacilli" are everywhere present, and if this is the case how are we to escape them, etc.? His use of the term, bacilli, in its generic sense is misleading; I can hardly think he meant that *tubercle* bacilli were everywhere present, for this is not the case. They *may be* present everywhere that tuberculous individuals are present or have been present, but to say that they *are* present in all places does not accord with the results of observation.

DR. FRANK WOODBURY—The facts collected with so much commendable diligence by Dr. Flick are susceptible of quite another interpretation. They certainly do not prove consumption a contagious disease, or that the bacillus tuberculosis is its sole cause. If he had confined himself to the task of showing that want of sunlight, dampness and other malhygienic conditions were efficient causes of phthisis his data would have been equally applicable. Or if he had intended to prove that the habitations of some of the poorer inhabitants of this city were likely to act injuriously upon the occupants, so as to predispose them to phthisis, we might have been willing to go with him almost to any extent in making application to the Board of Health to destroy these sources of disease. We might admit that by adopting such drastic measures the mortality from tuberculosis would undoubtedly be greatly reduced. The total extinction of pulmonary consumption, under present conditions of civilized life, however, is entirely impracticable.

It may be admitted, if the proposed plan of registration and inspection were rigorously carried out, that the results would apparently support the claims of the author. The number of cases reported would certainly diminish to a remarkable degree; but the real explanation, however, of the diminution would be that a large number of cases would be concealed, and deaths would be reported as from other causes. Moreover, a large number of cases would object to the solicitude of the Board of Health for their welfare and leave the city. Physicians would have their sympathies appealed to by relatives, who might wish to conceal from the patient the fatal character of the malady, and would be in a dilemma between duty to their patients and duty to the State. In fact, registration would eventually divide physicians into classes—those who reported their consumptive patients and those who did not. The physicians having the reputation of not reporting their cases would naturally have a larger clientéle than others.

The discussion of the registration of consumption manifestly brings up the question of the nature of phthisis. It is true that there are other clinical conditions which resemble tuberculosis of the lungs and which are not due to the presence of tubercle bacilli. Assuming, however, for the moment that all cases of consumption are tubercular, we may ask how large a part in disseminating the disease is played by contagion? We know that Dr. Brush calls attention to an infected milk supply as the principal source of tuberculosis in our cities. Bowditch showed conclusively that dampness of the soil is a very powerful factor in the causation of phthisis. Other authors claim that it is mainly due to inheritance. If these views may be regarded as well established, then the infection sinks to a secondary place. If tuberculosis is the *pébrine* of the human race, as has been stated, this invites a consideration of the subject from the standpoint of comparative pathology. If phthisis is analogous to the fungous diseases of the lower animals and plants, then the results of treatment in the latter may throw light upon the therapeutics of human tuberculosis. In a communication made by Dr. George B. Wood, in 1869, to the American Philosophical Society he referred to a discovery which he had made, and which, I think, has a very important bearing upon this subject. He proved by experiments upon a large number of fruit trees, which were prematurely dying by progressive decay (now recognized as some form of fungous disease), that by simply adding potash to the soil the trees were restored to their early vigor and they resumed fruit bearing; in fact, the crops were larger than ever before, "many branches breaking down with their load of peaches." This is a striking illustration of the importance of supplying to the soil some chemical agent which may be deficient, rather than devoting our time and energies to the destruction of germs. Pulmonary consumption is certainly curable. How many have been cured solely by an antiseptic or a parasiticide treatment? Not one; but many have been restored to health by hygienic methods alone.

Let me briefly quote, without comment, a case in point: It is that of a distinguished Fellow of the College, the late Dr. Joseph Parrish. Dr. George B. Wood, who delivered the obituary address before this College, thus alludes to Dr. Parrish's illness: "When about twenty-five years of age the doctor was affected with a severe and lasting cough, and considered himself in danger of pulmonary consumption, to which he believed that he had a family predisposition, having lost a brother and sister by that complaint. Under a course, however, of vigorous, exercise, and from exposure to the air, without the use of medicines, he ultimately surmounted the threatening symptoms. The existence of cicatrices in the upper portion of the lungs, discovered upon postmortem examination, proves that his apprehensions were well founded, and at the same time affords strong evidence in favor of the plan of treatment which he adopted in his own case, and always strongly advocated."

Recent investigations into the etiology of tuberculosis seem to lead us away from the germ theory, and to point to the fact that in the tissues of the phthisical patient some chemic agent is deficient, which if we can supply, the symptoms will disappear and the case will proceed to recovery. This agent may not be the same in different patients. Cases of consumption have been cured by the hypophosphite treatment advocated by Churchill; others have been restored to health by alcohol; yet others have been equally improved by drinking blood at the abattoirs, or by eating raw beef. Codliver oil, advocated by Bennett, seems to supply the needed element in some; an abundant milk-diet has been successful in others. The agent may be a simple salt-like lime hypophosphite or sodium phosphate, or organic in its nature like tuberculocidin or nuclein. Whatever it may prove to be, one thing is certain, that the results from following out this lead have been more successful than those from parasiticide treatment. They do not strengthen the argument of those wishing to class tuberculosis among contagious diseases.

In conclusion, I might point out the fact that if the Board of Health should force physicians to make diagnoses this would involve the compulsory use of all modern methods, including the microscope and culture tubes. The fact has just been announced that ex-Vice-President Morton's fine herd of Jersey cattle, recently exhibited at the World's Fair, has been found to be infected with tuberculosis. It is said that the animals presented no symptoms of disease whatever, and the diagnosis was made by the subcutaneous injection of tuberculin. Can physicians be compelled to resort to this in all suspected cases in the human subject?

DR. SOLIS-COHEN—The question to be considered is simply this: Granting all that may be said concerning the etiologic relations of the bacillus of Koch to tuberculosis, and granting all that is said as to the spread of infection from persons affected with tuberculous disease—on the whole, will the health and comfort of the community, including both the well and the sick, be better promoted by having all cases of tuberculosis registered or by not having them registered? So far as the sick are concerned, the disadvantages of registration are obvious. So far as the well are concerned, my distinguished teacher, the Vice-President of the College, has shown that the reasons advanced in support of the proposition that it is necessary to have cases of tuberculosis registered in order to protect the health of the community, can not withstand critical analysis. I believe, moreover, that registration would be a detriment to the community, for the reason that when of two measures to prevent an evil, attention is concentrated on the less efficacious, the more efficacious is likely to be neglected. If we direct our attention too strongly toward germicidal measures we shall lose sight of the more important measures that relate to the general sanitation of cities, of houses, and of individuals. Tuberculosis among men is to be prevented by increasing the vitality of the race, by preventing the marriage of tuberculous and otherwise unfit persons, and by perfecting the sanitary environment and life of the individual. The true prophylaxis against tuberculosis begins, therefore, before conception; but as society has not reached the stage of development where such prophylaxis can be generally insti-

tuted, it becomes all the more necessary to insist upon those practical measures of individual and civic hygiene which must be our greatest dependence and to oppose whatever shall tend to obscure their importance.

As has been said by Dr. Woodbury, if Dr. Flick had started out to prove that consumption flourishes in overcrowded places and where misery, poverty, filth, and vice are congregated, his statistics could not be more admirably adapted to enforce that conclusion; but to prove that tuberculosis is contracted only or chiefly by living in houses in which persons having tuberculosis have previously resided, would require that a census be taken to find out concerning every house in the city whether at any time there had been a patient with tuberculosis living or dying in the house, and if so, how many cases had followed and at what intervals, and also to determine how many cases of tuberculosis had developed among those living in houses where no one having tuberculosis had previously resided. Until statistics of this character are at hand, the case against the so-called infected houses, in districts where the city, the houses, and the lives of the people furnish so many potent cases of impaired vitality, is at least "not proven." But grant that the proof of infection in houses is conclusive. Suppose that the houses are thoroughly disinfected, and even, referring to Dr. Wood's point, guarded against reinfection—what have we gained? We are told—and I invite correction from Dr. Abbott, if I am in error—we are told by the highest authority that the bacillus of tubercle is about us everywhere. It exists, save at certain altitudes, or upon the sea, or in other specially favored regions, in the air we daily, momently breathe. Every one is at some time exposed to the danger of inhaling it, and that it is inhaled by everybody is shown by the results of autopsies upon persons having no tuberculosis elsewhere, but in whom the bronchial glands are infected with the tubercle bacillus. I believe that, according to some statistics, the proportion of persons so infected has been placed at 50 per cent. Is it not clear that, as was said by the President of the College in his Annual Address, in order to make the Board of Health laws effective, there would have to be instituted a system of espionage so that the tuberculous patient could be followed every day from the time of his rising up to that of his lying down, while seated in his house, or while walking by the way, to make sure that by no possibility should his sputum be deposited into any place where it could become dry and be carried into the air—a thing which is manifestly impossible. But if instruction only is to be given and no such rigorous supervision of individuals attempted, it is within the province and within the power of the attending physician to instruct his patients, and the intervention of the municipal officer is not required.

Therefore, because of the impracticability of this registration to effect anything positively, and because, negatively, it will have the effect of diverting attention from the fact that these streets or alleys, to which Dr. Flick directs our attention, should be cleansed and widened, the houses properly drained and ventilated, the people properly fed and clothed, and otherwise protected from exhausting and depressing influences, I consider it to be the duty of the College to place its strong word on record against the proposed legislation.

Were the proposition in favor of a municipal hospital for tuberculosis, separated from the other recommendation, and properly safeguarded, I should be glad to vote for it.

Dr. Richard A. Cleeman—One very important point has not been alluded to. Statistics show that the death rate from the disease is almost the same in every community, year after year. If there is any diminution, it is very small. This is directly opposed to the idea of the contagiousness of the disease. All other contagious diseases vary very much in their death rate at different times. There is only one explanation by which we can conceive that phthisis is contagious and the death rate always the same, and that is, that the whole of the community is thoroughly infected with the tubercle bacillus, and that in the community there is a fixed number who are susceptible to the disease. Just, as statistically, we can calculate how many giants or how many dwarfs are in a community, so can we estimate the number of persons susceptible to phthisis.

In this view of the case, with the bacillus tuberculosis spread over the whole city, measures to be effective must be of a very extensive character. The mere reporting of a case now and then—for I believe the rule will be often neglected—will have very little effect. Measures to be effective must be thorough and they are the methods of general hygiene.

Dr James B. Walker—We must look at the practical side of this matter, and ask what will be the benefit from registration? How much has typhoid fever in this city been diminished by the fact that we report our cases? Not in the least. The disinfection of the stools which the physician has directed has lessened the number of cases, as similar attention to the sputum may in phthisis. I am against placing phthisis on the list of diseases to be reported, for many reasons. I do not believe that this would confer a single benefit upon the city or its inhabitants. The physician is capable of doing all that the Board of Health can, without the manifold evils and annoyances of public registration. The College should go on record against the registration of the disease. It should go on record also as believing that the disease is possibly contagious, and should recommend physicians to use the most strenuous efforts to prevent the transmission of the contagion.

Dr. Da Costa—I used the expression that the tubercle bacilli were everywhere. A bacillus that is on the pavements and in houses, that is wafted about in the air, that exists in milk, and on fruit sold in the markets and, as has recently been demonstrated, is on the very money that we handle, may well be described as a bacillus that is everywhere. The tubercle bacillus is so diffused that it can, indeed, be considered as everywhere present, and it is scarcely a figure of speech to so describe it.

With every word that Dr. Roberts has said, I agree. There is not a direction he has read I would not subscribe to and endeavor to enforce as an officer of an institution, or in private life. I hope, indeed, that every Fellow will go away to-night imbued with the necessity of carrying out all measures that have been brought forward with the view of destroying this scourge. But is it not better that we should do this as individual health officers, than to have it done in a necessarily perfunctory way by a Board of Health? The only way that the Board of Health could radically act to destroy the tuberculous material would be to burn every house that was supposed to be really infected, separate the tubercular husband from the wife, take every child away, and cause such an upheaval in society as no community would submit to.

Dr. Flick—Dr. Williams of Brompton Hospital, has been quoted as an opponent of the theory of contagion, but he has written a paper in which he shows that a large number of nurses of the Brompton Hospital have contracted tuberculosis. I think that he has traced some fifteen or twenty cases.[1]

As to the ubiquity of the tubercle bacillus, Koch and Cornet have disproven it. They have shown that the contagious environment of tuberculosis is limited. Cornet has made experiments with dust taken from the streets and other places at random, and was unable to produce infection by inoculation. He has, moreover, made an investigation of the health of the street-cleaners of Berlin, and has found them freer from tuberculosis than any other class. It must be borne in mind that the isolated cases of infection of fruit and other articles which have been referred to are traceable to handling by consumptives. Many of the fruit-stand dealers have consumption.

With regard to the endeavor to ascribe the occurrence of the disease in certain houses to dampness, that can be disposed of in a moment. If you will take one of the worst infected streets in the Fifth Ward, say Bay Street, you will find that at least 20 per cent. of the houses have not had a death from tuberculosis, although the sanitary condition is as bad as in the infected houses. Dampness can have no influence here.

As regards infection in hospitals, as I have said, Williams has written a paper in which he shows that fifteen to twenty nurses have contracted the disease in the Brompton Hospital. Cornet has written an elaborate history of hospital infection. He has taken the general hospitals of Germany, in which the nurses are permanent, and he finds that as high as 62 per cent. had tuberculosis. In my own class of residents at Blockley, sixteen in number, two died within three years of the disease, and I myself was believed for a time to be a victim.

It has been argued that we ought to trace the history of these houses for a longer period, say one hundred years. There is a limit to the infectious power of tubercle contagion. What that limit is I can not say; probably not very long. Unless cases occur within a few years of each other to keep up the chain of infection, the houses are no longer infected houses.

Italy has been quoted as an evidence of the non-effective-

[1] British Medical Journal, Sept. 30, 1882.

ness of preventive measures. I am surprised that this quotation is made so frequently after what has been written. Those who have seen my paper on tuberculosis in Italy will be in a position to refute this statement. I have proven in that paper, by careful quotations from contemporary authorities —and authorities that can not be doubted—that at the time when the laws were instituted in 1782 the mortality was about 10 per 1,000, and that when the preventive measures were abandoned, the mortality was so low in the extreme southern part of Italy that it was practically nil, 0.19 per 1,000. Instead of there being no result, the mortality was reduced from 10 to a fraction of 1 per 1,000. Many who have written upon the subject have overlooked the fact that it is not Naples but the Kingdom of Naples that is spoken of. This comprised the southern half of Italy and Sicily. At first the northern part of Italy opposed the view of contagion and refused to accept any preventive measures. After some years it adopted preventive measures, but even at this day the mortality in the northern part of Italy is twice what it is in the southern part. These preventive measures proved another point, and that is the local infection. The only practices that were of any benefit in the Kingdom of Naples were first, isolation; and second, compulsory disinfection. The preventive measures circled entirely about the household. The Italians did not know where the infection came from, but adopted crude measures of prevention. Yet with their crude measures they reduced mortality to a minimum, and except for the fact that Italy became a health resort for tuberculosis, it probably now would have no tuberculosis. If you will study the statistics you will find that those parts of Italy that are health resorts for tubercular patients run up the mortality. This has been as high as 4 per 1,000 in some of the resorts, but if you take the rest of the Kingdom of Naples, the mortality is a fraction above 1 per 1,000.

As to the competency of the physician to meet this question: In connection with this subject, we must bear in mind that every infectious disease is a law to itself. It is not fair to apply to tuberculosis the laws which govern diphtheria, smallpox, etc. The infecting material in tuberculosis is easily controlled. It comes from a single source. In smallpox it is difficult to control. If it were possible to sterilize all the sputa of consumptives we would not need disinfection. Such a thing is impossible, however, and therefore it is necessary that the room be disinfected. This requires great care, and unless the family is willing to undergo the expense it can not be accomplished. If the physician were to devote half of his time to bring about proper disinfection he could not do it effectively.

Dr. Cleeman has advanced an argument against contagion which, strange to say, has been made for the other side by Dr. De Forest. Dr. De Forest claims that inasmuch as the number of infected houses is limited, the mortality rate should remain the same every year, because the number of exposures remain the same. The centers of infection remaining the same, the deaths should remain the same.

Tuberculosis and typhoid fever have been compared. The reporting of typhoid fever can not bring about any results unless the lessons taught are acted upon. If the water supply were made better we should have no typhoid fever. There is no analogy between typhoid fever and tuberculosis. Tuberculosis is infectious from the point at which it exists; typhoid fever infects our drinking water. There is little danger from the room occupied by the typhoid fever patient. The cases are not at all analogous.

DR. MAYS—When this question was discussed before the County Medical Society, about four months ago, I quoted Dr. Williams, the same authority to which Dr. Da Costa referred tonight in support of the non-contagiousness of the disease in the Brompton Hospital.

I read his conclusions from his book, "Pulmonary Consumption," pp. 87–89, published in 1888, in which he emphatically states that contagion in the wards of this Hospital is absolutely unknown.

In concluding the discussion of that evening, Dr. Flick said that in a private communication, which he had not with him, but which he received from Dr. Williams since the book I quoted from was published, the latter admitted that his ideas had undergone a change in this matter, and that now he believed that about thirty or forty cases of phthisis had arisen in the Brompton Hospital through contagion, during the time over which his research extended. This seemed to me so extraordinarily strange and surprising, for, besides the conclusions set forth in Dr. Williams' book, I heard him say in one of his lectures at the Brompton, in 1883, that not a single clear case of contagion had occurred in the wards of that institution, that I determined to write him and ask for the cause of this change of opinion on his part. I received his reply last October, in which he states that he has not changed his views on the infectiousness of phthisis materially, since he summed them up in the second edition of his work on "Pulmonary Consumption," pp. 87–89 (precisely that which I quoted), and that there are certainly no records to support the statement that thirty or forty cases of infection occurred in the wards of the Brompton Hospital. In fact, he says: "The evidence of the Brompton Hospital is all against infection, but we ought to admit that infection is possible through dried sputum under very exceptional circumstances, but that the slightest attention to cleanliness precludes any chance of this infection."

In this evening's discussion Dr. Flick again refers to this private evidence which he received from Dr. Williams, but he now states that instead of thirty or forty cases of phthisis which could be traced to infection in the Brompton Hospital, during the time referred to there were only fifteen or twenty. It seems to me, therefore, that for the sake of scientific accuracy, and for the purpose of enlightenment, Dr. Flick ought not to withhold from us any longer the information which he claims to possess on these contradictory points.

DR. FLICK—The statements which I made are contained in an article by Dr. Williams, which appeared in the *British Medical Journal*, Sept. 30, 1882.

Dr. Charles W. Dulles—This is a discussion in which exactness is of great importance, and in which approximate figures and inferences must not be permitted to take the place of actual numbers and of facts. The danger of venturing on unfamiliar ground is exemplified in the comments on what Dr. Cleeman has said in regard to typhoid fever. Those who have specially investigated the sanitary condition of Philadelphia and the condition of its water supply know that what Dr. Cleeman says on these subjects can be relied upon, for he has given to them unusual and unusually conscientious study.

Although when I came here I had no idea of speaking, I think it might be said for some who have so far kept silent, that we do not tonight hear for the first time of Cornet, or of the Brompton Hospital and Dr. Williams, or of the Ventnor Hospital. The facts and theories emanating from these are all things about which we know something and about which we have already opinions.

In regard to the improbability of the effectiveness of measures of inspection instituted by the Board of Health, I agree with Dr. Walker. We have all probably had personal experience of the unsatisfactory way in which the Board of Health carries out its regulations in regard to diseases now returnable as contagious.

In passing upon the question before us, I feel sure that the College will act in keeping with its traditions of a hundred years, and give a new illustration of the fact that it is a body in which prudent and conservative counsels prevail, and I have no fear that it will hastily commit itself, as a sister society has done, under the ingenious arguments of those strongly infected with the idea of the infectiousness of tuberculosis.

A vote was then taken on the amendment offered by Dr. Flick; it was lost, and the original resolutions, as proposed by the Council, adopted, the President being requested to appoint a committee of five to present them to the Board of Health.

SOCIETY PROCEEDINGS.

American Electro-Therapeutic Association.

The Third Annual Meeting Held in Chicago, Sept. 12, 13 and 14, 1893.

AUGUSTIN H. GOELET, M.D., President.

(*Continued from page 168*).

SECOND DAY, SEPTEMBER 13—AFTERNOON SESSION.

The meeting was called to order at 3:10 P.M.

THE INFLUENCE OF FREQUENCY OF INTERRUPTIONS AND CHARACTER OF INDUCED CURRENT WAVES UPON PHYSIOLOGIC EFFECT.

Discussion opened by WILLIAM J. MORTON, M.D.

Some months since, when I gladly consented to open this discussion, I had made arrangements with a distinguished electrical engineer to experiment in his laboratory with periodic currents in relation to the frequency and the character of the induced current wave, but his illness has rendered this impossible.

I must content myself, therefore, at present with a very brief opening.

For convenience, at the outset, we may put the question in a simpler manner and ask, what is the physiologic effect of induced currents, referring to induced currents which have not been rectified into continuous currents, but still retain their fundamental periods, whether these be alternations or intermittences. An attempt to answer this general question is facilitated by an analysis of induced currents into two of their prominent characteristics, designated by the title of this discussion, namely: *a*, the frequency of their periods in a given time, for instance, per second; *b*, the character of the induced current wave.

The frequency of the periods of the induced current is originated in two ways:

1. By mechanical devices, as in the induction coil and in the alternating and intermitting dynamo-electric machine.

2. By a natural law of electro-physics as in the oscillating discharge of condensers:

The character of the induced current wave is identified by mechanical devices which record the rise and fall of its potential and other of its characteristics, upon the Marey registering drum or other registering apparatus.

We have, therefore, at our disposal three mechanisms, artificial or natural, or both combined, for exciting induced currents; these are: 1, the induction coil; 2, the dynamo-electric machine; 3, condensers (Leyden jars, etc.)

1. The induction coil.

The induction coil has long been in medicine a therapeutic means of exciting physiologic effect. This effect in the main has been to stimulate the motor and sensory nerves, namely, to produce powerful muscular contractions and to produce pain. In recent years physicians have given attention to improving the induction coil and we have heard much about the "long and fine wire" and the "short and coarse wire" secondary windings and their effects. But such modifications as regards the length or fineness of the wire of the secondary coil is of little account in advancing our knowledge of induction coils. For, in fact, familiar as this little instrument is to all of us, it is yet full of difficulty and unsolved problems. Elements of difficulty are the "lag" between the primary and secondary, the self induction in each, the effect of the magnetic resistance of the iron core or magnetic "lag" and the difference in the wave-curve of magnetization and demagnetization, and the irregularities of the vibrator; again, the frequency of the interruptions of the current in the primary circuit is limited by reason of the delay in magnetizing and demagnetizing the vibrator, ranging from 200 to 2,000 per second. We are furthermore ignorant of the character of the induced wave of the induction coil.

I find a record of the graphic curve of coils in the *Electrical Engineer* of Aug. 3, 1892. The experiments were made with the apparatus of Dr. Frolich of Messrs. Siemens & Halske of Berlin. The induction coil was so arranged that the core which consisted of a bundle of iron wires, could be removed at will. The two windings were precisely similar and consisted of 4,900 turns of wire, the resistance of each being 200 ohms.

CORE REMOVED.

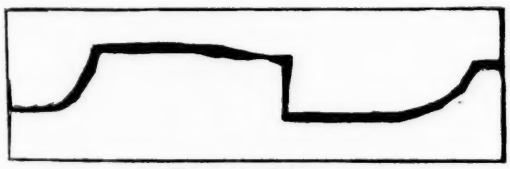


Figure 15. Primary.

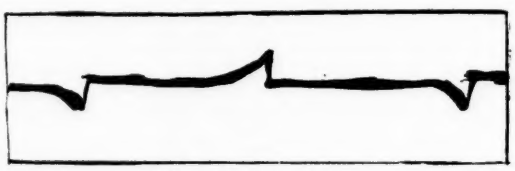


Figure 16. Secondary.

Fig. 15 shows the curve of the current in the primary winding with core removed, while the secondary current is shown in Fig. 16. In this case the primary current attains its maximum quickly while the induced secondary, changes as quickly as did the current in the case of a condenser without resistance.

CORE IN POSITION.

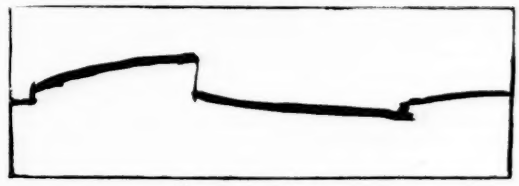


Figure 17. Primary.

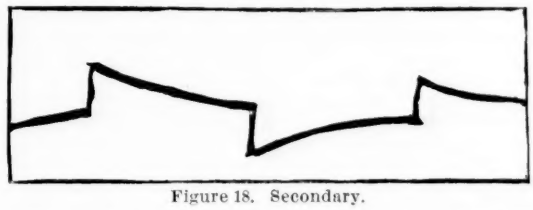


Figure 18. Secondary.

Figs. 17 and 18 represent the primary and secondary respectively, with iron core in position. The effect of the core is very marked. The primary current gradually attains its maximum on making the battery current. By breaking the battery current and short-circuiting the primary coil on itself the current gradually falls to zero.

The secondary does not, as in the case without the core, suddenly fall away, but as one would expect, it gradually falls to zero.

In these experiments the time of a complete impulse was from 1-10 to 1-20 second.

It is a matter of congratulation that we shall have a paper upon induction coils presented before us by so able an electrician as Mr. A. E. Kennelly. What he has to say will fill many gaps left open in this discussion.

Medically we have apparently reached, and long ago, our limits in the effects to be expected of induction coils; we stimulate motor and sensory nerves, we increase metabolism, to the extent that muscles, caused to contract, increase it; we do not yet get pronounced sedation unless Dr. Hutchinson can demonstrate his electrical anesthesia.

To increase the precision and usefulness of periodic currents in medicine we must carefully study the form of the current wave. We must turn to other apparatus, other electrical or mechanical devices. This is what Prof. A. D'Arsonval of Paris, has done and with remarkable results, namely, the development of the sinusoidal current in new and unexpected relations to physiologic effect and its introduction into medicine.

Desiring to get a true sine curve he discarded iron in his armature and caused a magnetic field to be revolved close to a stationary wire coil without iron core; desiring to increase his frequency of alternations he was enabled to do so with the same mechanism by increasing the rate of its revolutions. He was able to reach a speed of 10,000 alternations per second.

And this is what he has taught us of the sinusoidal current of low frequency, (50 to 60 per second). The nerve and the muscle are not excited, there is neither pain nor muscular contraction, but a remarkable effect occurs, the nutritional processes of the subject are stimulated, the internal respiration or interchange of material between the blood and the tissue is increased to the extent that the amount of oxygen consumed is augmented by nearly 50 per cent. while there is likewise a correlative increase in the carbonic acid and urea excreted.

And, concerning the physiologic effects of increasing the frequency, the following interesting data were established. At 20 to 30 excitations the muscle is tetanized; the tetanization reaches its maximum at 2,500 to 5,000 excitations per second, and from this point the energy of the muscular contractions gradually diminishes up to 10,000 per second where it is *nil*.

In other words the neuro-muscular excitability, after a certain maximum is reached diminishes with the increase in the frequency.

Now this is a remarkable fact, and one which the expert electricians, interested in developing the resources and particularly the *safety* of industrial alternating currents had not been slow to observe. The fact may be expressed crudely in this way. Of two currents which are delivering exactly the same total amount of energy to a living person or animal, the one an alternating current infrequently alternating (100 to 10,000 per second), the other frequently alternating (10,000 to 1,000,000 per second); the former or

the one with long periods would instantly kill the person while the latter with short periods would be harmless, or indeed if carried to the highest frequencies would produce no sensations whatever.

It is here purely a question of *physiologic* effect for these same innocuous currents exhibit torrents of sparks, cause incandescent lamps to glow and perform other mechanical work. But the nerves and the muscles are not excited.

In one of D'Arsonval's experiments six lamps of 125 volts and 0.8 ampères, were brought to incandescence held in a circuit formed by the arms and person of the subject, the latter experiencing no sensation due to the passage of the current. The amount of energy traversing the body was 900 volts and 0.8 ampères = 720 watts.

But the facts of the experiment have led us a little in advance. Prof. D'Arsonval, as has been seen, did not cease his labors with a frequency of 10,000 per second.

His next step was to go beyond this frequency. And in taking this step he approached the labors of others, like those of Elihu Thomson, Dr. Tatum and Nicola Tesla, with a remarkable simultaneousness in time, and some of them like those of Morton, anticipatory in some essential features by thirteen years. He stepped into the region of high frequency high potential currents.

A brief glance at the historical sequence, the mechanisms and the physiologic effects of each observer will here suffice to point out the interesting and new field laid open to electro-therapeutics by the changes in the element of frequency as well also as in that of the graphic curve, could that always be ascertained.

The high frequency high potential current by aid of discharging rods and a condenser in the primary circuit was first discovered by Dr. W. J. Morton in 1880. His mechanism, then published, is the fundamental electrical mechanism necessary to produce these currents, now so thoroughly familiarized by the labors of Tesla, Elihu Thomson and D'Arsonval. In a paper read before the New York Academy of Medicine, March 3, 1881, and printed in the *New York Medical Record*, April 12, 1881, Dr. Morton described "A New Induction Current in Medical Electricity," derived from an influence machine, (Holtz machine). The current was obtained by the aid of Leyden jar condensers, one attached to each prime conductor. Rheophores and wet sponge electrodes were connected to the outer coatings of the jars and the discharging rods were approximated without quite touching. Upon actuating the Holtz machine a person holding the electrodes received a peculiar current causing widely diffused muscular contractions with a minimum of pain to excessive currents followed by a marked analgesic effect in the parts treated. These effects were carefully noted. Again in December, 1890, Dr. Morton read a second extended paper upon the same subject before the New York Neurological Society. (*New York Medical Record*, Jan. 24, 1891.)

The following diagram among many others was used to illustrate the connections to produce this high frequency high potential current:

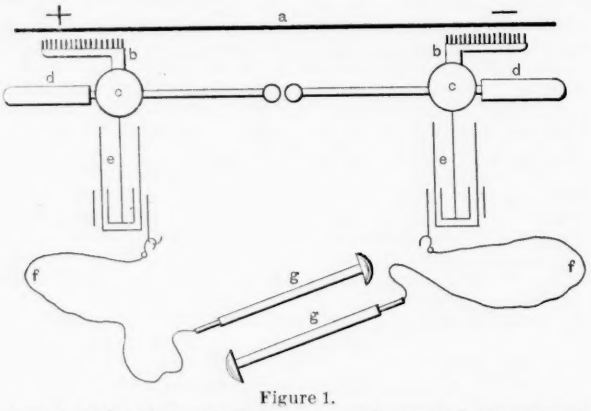


Figure 1.

"Static Induced Current." Person, condenser and circuit-breaker in same circuit, connecting-rod between condensers removed, and discharging-rods of machine serving as circuit-breaker. The make and break in the primary is accompanied with a current in the secondary.

The interesting feature of this publication is that in 1881, Dr. Morton had already discovered the method of producing a secondary current of high frequency and high potential by means of a primary circuit which included discharging rods (with air gap) and condensers in this circuit, and had pointed out peculiar physiologic effects.

Referring to the sensory effects of the current it was remarked: 1, that it fails to excite, or may even annul, to an extent, the sensation of pain and that there was experienced a peculiar sensation of lightness and buoyancy and a feeling as if the arms were made of cork, while of the effects upon the muscles it was noted that: 2, a great diffusiveness of the action, whereby large groups of muscles were simultaneously caused to contract with a minimum of pain.

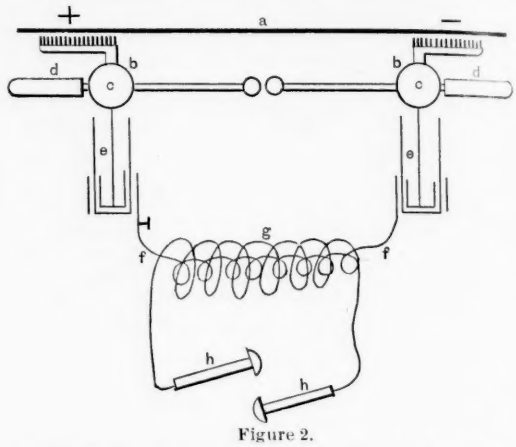


Figure 2.

Figure 2 represents one form of Dr. Morton's transformer. A primary wire extends between the two outer coatings of the Leyden jars and this primary is surrounded by the secondary. Oil insulation is used. This apparatus was constructed many years ago and in several forms by the Waite & Bartlett Manufacturing Co. of New York, but was only described publicly in a paper read at Columbia College, before the New York Electrical Society and the American Institute of Electrical Engineers, May 17, 1893. This paper has appeared in their transactions.

Dr. Morton also exhibited at the Columbia College meeting alluded to, the illumination of vacuum lamps, having one entering conductor and a second of tinfoil upon the outside of the globe.

Tesla's first publication was in the *Electrical World* of Feb. 21, 1891, under the title of "Phenomena of Alternating Currents of very High Frequency." He made this interesting concluding observation: "The writer's experiences tend to show that the higher the frequency the greater the amount of electrical energy which may be passed through the body without serious discomfort."

Mr. Tesla's mechanism consisted of a high frequency dynamo-electric machine whose current enters a primary wire, a secondary wire within whose circuit is included a condenser and discharging rods, and again a coil to which a part of the secondary wire acts as primary.

The current produced after this special method has not been tested in medicine.

Prof. Elihu Thomson at just about the same time was engaged in studying the physiologic effects of increasing the frequency of the periods. His apparatus consisted of an alternating dynamo machine having 800 armature coils and capable of producing 8,000 alternations per second. This machine was intrusted to Dr. Edward Tatum of Yonkers, N. Y., who conducted extensive physiologic experiments with it.

Dr. Tatum's report, embodied in a letter to Prof. Thomson, Dec. 29, 1890, and published in March, 1891, brought important facts to view, and deserves much higher recognition than it has received. He established:

1. That the fatal effects of alternating currents upon animals were in inverse ratio to the frequency.
2. That the cause of pain which limits voluntary toleration lies chiefly in the muscular contraction produced.
3. That the cutaneous nerves were distinctly less painfully affected at the high rate.
4. That the visual mechanism was not excited at the high rate even by a pressure of 15 volts so as to cause a sensation of flashes of light, although two or three volts similarly applied at 120 oscillations caused such flashes very energetically.
5. That the sensation of vertigo produced by the low rate was not produced by the high.
6. That sensations of taste were absent to the high rates.

7. That exposed motor nerves of frogs were less excitable to the high rate than to the low rate.

The fact that when the number of oscillations is extremely high there is no excitation of the nerves and muscles, D'Arsonval communicated to the Biological Society of Paris, Feb. 24, and April, 1891. His recent conclusions as to the physiologic effects of high rates of frequency are:

1. No effect upon the organs of feeling.
2. No muscular contraction.
3. Diminution of the sense of pain.
4. Dilatation of the blood vessels.
5. Increased perspiration.
6. Increased tissue change, manifested by increased absorption of oxygen and increased elimination of carbonic acid.
7. No increase of bodily temperature.

As to why periodic currents excite nerve and muscle in inverse relation to their frequency, we are confronted thus far, for answer, only by hypotheses. Prof. Houston believes it is because currents of this nature do not sufficiently penetrate beneath the surface of the conductor. Prof. D'Arsonval offers as explanation that the nerves and their centers, if not all the tissues, respond only to excitations of a certain frequency and that, therefore, if these currents fail to excite pain or contraction, it is because their periods do not coincide with the periods of the nerves producing pain and contraction.

Other explanations not here essential to adduce have been offered.

It will, however, be borne in mind that though nerve and muscle may not be stimulated so as to produce pain or contraction, still there take place other effects of far higher importance, viz.: stimulation of the nutritional processes of life.

Dr. Morton then exhibited a medical induction alternator devised by Mr. A. E. Kennelly at his suggestion.

This small medical alternator has twelve poles; each pole is wound with a primary and secondary winding on one spool. The exciting current in the primary circuit is less than one ampère; frequency, twelve periods per revolution; speed attainable, 6,000 revolutions per minute; frequency attainable, 1,200 periods per second; armature of laminated iron plates, running in self-oiling bearings; field frame, laminated sheet iron supported in cast iron guides.

Dr. Morton also presented a wheel, or mechanical means of securing sinusoidal interruptions from galvanic batteries, and for operating faradic machines, devised by Mr. R. G. Brown, electric engineer, for him.

The apparatus consists, as you see, of some forty contacts or segments arranged in a circle and insulated one from the other. Projecting through the center of the circle there is a shaft carrying an arm to the end of which is affixed a small, flexible wire brush, so adjusted as to rub over the face of the segments. On the end of the shaft there is a pulley wheel, and this was connected by belt to an eighth of a horse power motor making 1,500 revolutions per minute. Now, beginning at one point of the circle and numbering all the segments from one up, all those of even numbers were connected together and carried to a positive battery, while those segments of uneven numbers were similarly connected to a negative battery—each of three cells Leclanchè. One end of the primary coil was then connected to the trailing brush and the other end of the coil connected to the other end of the batteries. It will be readily seen, that as the trailer rotates over the forty contacts or segments, this primary coil will be successively charged with positive and negative currents—20 + and 20 —, and as the trailer is making 1,500 revolutions per minute, we have 40 times 1,500, or 70,000 alternations, *each* of *equal* duration, passing through this primary coil.

Another experiment was made, in which a single battery and only one-half the number of segments were employed. The segments of even numbers were connected to one pole of the battery, the intervening segments being left disconnected. With this arrangement the action is similar to that derived where an ordinary vibrator is employed, *except* that the "make" and "break" is of equal duration.

No more important question has ever arisen in electrotherapeutics than the one presented under the title of this discussion. It is natural that considerable confusion will arise and many conflicting statements be called forth in this, the first attempt to elicit a general expression of opinion upon a subject still in embryo in its physical, and equally undeveloped in its physiologic principles. We can only begin this work at this meeting.

Heretofore with a long fine wire coil, patients have obtained relief in a great variety of cases. Dr. Morton's personal experience with the sinusoidal current entirely verifies the claims made for it by D'Arsonval that in this current electro-therapeutics gains the most important addition made to it in recent years. And he believes that the sinusoidal apparatus must eventually, to a great extent, replace the induction coil.

Mr. A. E. Kennelly's paper on

INDUCTION COILS,

was then read by Mr. E. M. Smiles.

Induction coils are instruments in which electro-motive forces are generated by the variation of their permeating magnetic flux. They were first constructed by Faraday in England, and Henry in America, and are now familiar to us under numerous variations of form and design; but with little variation in their fundamental principles, which we propose to here consider with special regard to apparatus employed in electro-therapeutics.

Every magnet, or every current-conveying helix acting like a magnet, has, we know, its complete magnetic circuit. In other words, magnetic flux knows no terminus, and is arrested by no barrier, but always appears in closed paths or reëntrant channels. If a small compass needle could be advanced along the direction that it selects from instant to instant, it would in any constant magnetic system, travel round and round some loop in space, unless arrested in its passage by geometrical barriers such as the iron core of an electro-magnet. By tracing out a sufficient number of such loops or flux lines in different portions of the magnetic field, we should come to regard the magnetic circuit as resembling a bundle of numerous separate and invisible endless chains densely packed within the magnet, but spreading out beyond its ends.

An electrical conductor, e. g., a copper wire, encircling all the flux lines of such a magnetic circuit without threading through any, is apparently unaffected by their vicinity, but so soon as it passes once (or some odd number of times) through flux lines, thus becoming linked with them, its electrical condition is influenced by their presence. No change can be detected in the wire, so long as it remains at rest relatively to the flux chains, but should motion take place between them in such a manner that the wire becomes linked with more or fewer chains, or should the magnetic circuit so alter as to increase or diminish the flux lines linked with the wire, an electro-motive force becomes developed along the conductor during the motion. Increasing the linkages of flux develops an electro-motive force in one direction through the wire, while decreasing the linkages sets up the reverse. The electro-motive force will produce a current of electricity through the wire if the circuit of the latter be complete.

The magnitude of the electro-motive force developed in such cases is readily determined. Suppose a copper wire to be bent into a loop with its ends in close proximity, but not actually in contact, so that no current can be sustained in the wire. If we take this loop from some position in which it was linked with 50,000,000 units of magnetic flux (50,000,000 of unit magnetic chains), and deposit it in such a position that it is linked with 150,000,000 units, then the increase in the flux linkages to the extent of 100,000,000 units will establish an electro-motive force in the wire during the transfer. Assuming that the operation lasted for one second of time, and was so controlled that the rate of flux admission or linkage increase was uniform, then the electro-motive force in the loop would be maintained at one volt throughout the motion, for by definition the volt is the electro-motive force induced in an electric circuit when magnetic flux enters it at the rate of 100,000,000 of units (C. G. S.) per second. Had the transference been effected in the one-thousandth part of a second, the rate of linkage would either have been uniform during that time, and equal to 100,000,000,000 units per second, or variable but averaging this amount, so that the electro-motive force induced in the loop would either have been steady at 1,000 volts, or variable but averaging 1,000 volts during the same interval. But although the magnitude and the character of the induced electro-motive force thus depends upon the manner in which the transfer takes place, the total quantity of electricity that this electro-motive force can deliver through a constant resistance is unaffected by the circumstances of the motion. For if the ends of the looped wire are connected through a fixed resistance, a rapid transfer will induce a large electro-motive force, and accordingly strong current of brief duration, while a slowly executed trans-

ference would entail a small electro-motive force and a feeble current through a correspondingly longer time. For a given admission or withdrawal of flux through the closed conducting loop, a definite quantity of electricity must flow round the latter, depending only upon its resistance.

These principles may be applied directly to the analysis of the ordinary Ruhmkorf or faradic coil. We may suppose this to be of the DuBois Raymond type, in which the outer and secondary coil slides freely over the inner and fixed primary coil. To simplify matters at the outset, we may first suppose that, contrary to usual practice, currents are supplied to the primary coil in such a manner that the flux in the magnetic current rises steadily and falls at a uniform rate, and with a definite number of alternations per second. The maximum flux through the magnetic circuit of an ordinary small faradic coil is about two thousand units, so that if this flux made 500 alternations or 250 periods per second, the condition of the magnetic circuit in regard to time may be represented by the zig-zag line in upper Fig. 1, oscillating between 2,000+ and 2,000— in the five-hundredth part of a second, corresponding to a rate of change in flux of 2,000,000 units per second. A single turn of secondary conductor closely surrounding the primary coil and core at the center of length, would develop, if linked with all these flux lines, an induced electro-motive

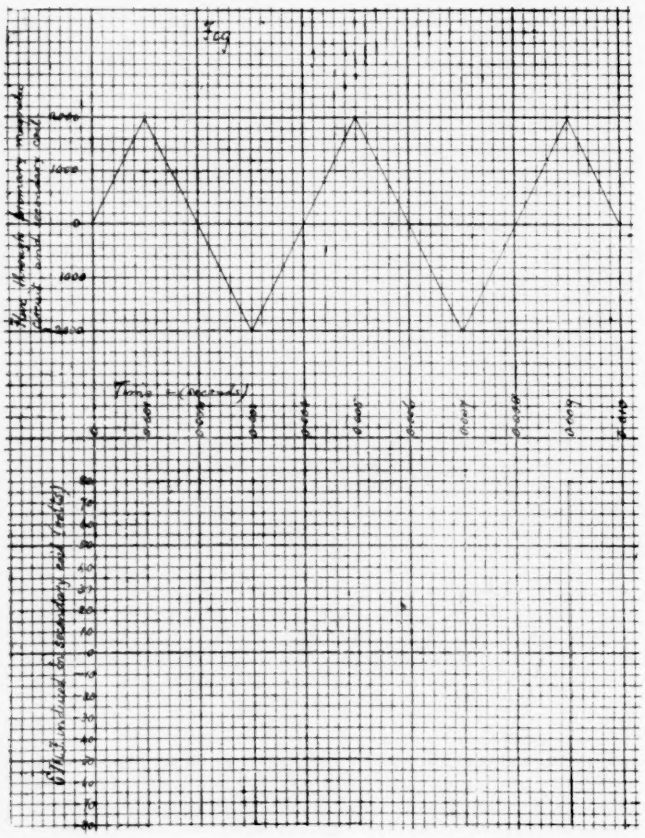


Figure 1.

force of $\frac{2000000}{10000000000} = 0.02$ volt, and suppposing that the secondary coil consisted of 4,000 turns all embracing the same flux, the total electro-motive force induced in the coil would be 80 volts, alternately positive and negative at a frequency of 250 periods, as shown in lower Fig. 1.

When the secondary coil is entirely removed from the primary, the electro-motive force induced in it must disappear, and even when placed immediately over the primary in the most effective position, the electro-motive force developed in every turn will not be quite equal. The flux through the iron core diverges in snch a manner that the turns near the ends of the secondary coil are not linked with so much flux as those at the center and inner layers. This is shown diagrammatically in Fig. 2, where P, Q, represents a longitudinal section of the iron core through its axis, and A, I, B, C, K, D, E, I, F, H, M, G, the section of a secondary coil. The turns at the outer edges, AG and BH, have a much smaller share of the total flux linkage than turns at the center at L, K. If, then, in the preceding case the most favorably situated turns developed an induced electro-motive force of 0.02 volts each, the aggregate electro-motive force would be less than 80. volts.

It is evident, therefore, that the induced secondary electro-motive force for a given arrangement of core, primary, and winding space, depends upon the number of turns in the secondary coil. A large number of turns necessitates the use of fine wire of great length and high resistance, but these are incidental qualities only. Fig. 2 shows that with a given winding space or length of wire, there is a gain in electro-motive force, obtained by slightly piling up the turns at the center and denuding the ends of the coil, thus securing the maximum total flux linkage, but the advantage is not usually considered of sufficient importance to pursue.

We may now replace the arbitrary primary current supply with the usual battery and spring vibrator. The frequency of vibration will depend upon the length and cross section of the spring, the elastic quality of its material and the mass and form of its iron armature or head. These conditions determine the fundamental or free vibration, but we know that forced vibrations can also be produced, whose frequency depends upon the strength and graduation of the magnetic flux, the position of the contact point, the play of

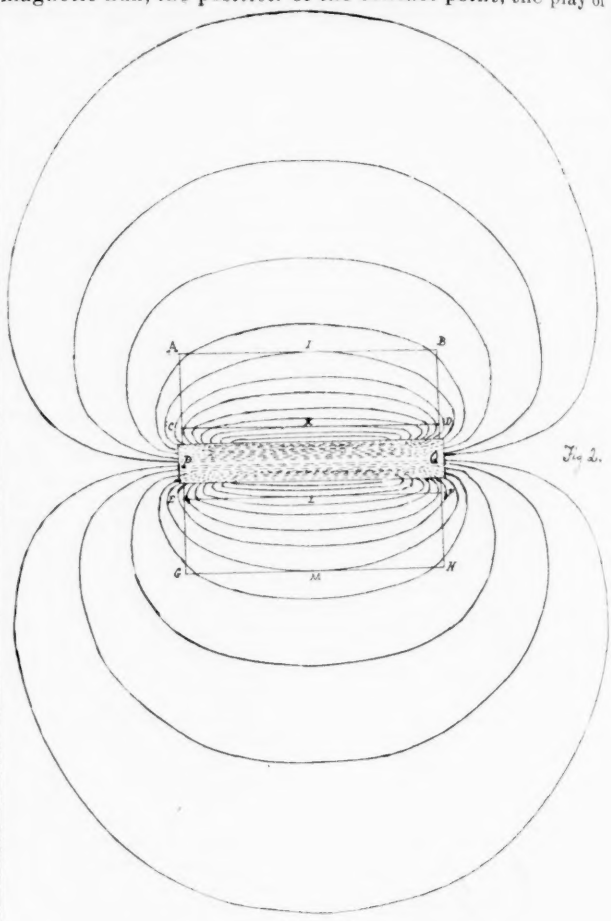


Fig 2.

Figure 2.

vibration allowed, and the tension imparted to the spring. Under any circumstances, however, the spring rests in contact with the platinum faced screw point during a certain fraction of each vibration, and the current from the battery finds access to the primary coil spring during this portion of the cycle. The primary current does not, however, instantly reach its full value, for that would entail a nearly instantaneous culmination of the flux, and a brief but enormously high induced electro-motive force in the secondary coil, contrary to observation. As soon as the primary current commences to flow, flux springs into existence in the magnetic circuit. This flux penetrates the convolutions of the primary winding, as well as those of the secondary, and consequently developes in the primary coil an electro-motive force always in opposition to that of the battery. This counter electro-motive force of self-induction retards the growth of the primary current and magnetic

flux, which then increases at a rate determined by the electrical conditions of the complete circuit.

As soon as the vibrator spring leaves the contact point, the primary circuit is metallically disconnected. If the primary current stopped instantly, the flux in the magnetic circuit would almost instantly disappear, entailing an enormously great but brief induced secondary electro-motive force in the opposite direction to that on making contact. This fails to occur, however, because the withdrawal of flux from the magnetic circuit not only induces electro-motive force in the secondary coil, but in the primary also, and in such a direction as to sustain the battery current. A spark therefore forms at the break, and through the column of heated air the primary current is for a short interval sustained. The result is that the flux is withdrawn from the circuit not instantaneously, but more rapidly than it entered, and it is well known that the induced electro-motive force of "breaking" is much greater than that at "making."

These conditions for the case of a vibrator making 100 periods per second is shown diagrammatically in Fig. 3. The broken line ABCDEFG represents the relative magnitudes of the primary current and the circuital flux during 0.016 second or about one cycle and a half. The vibrator spring

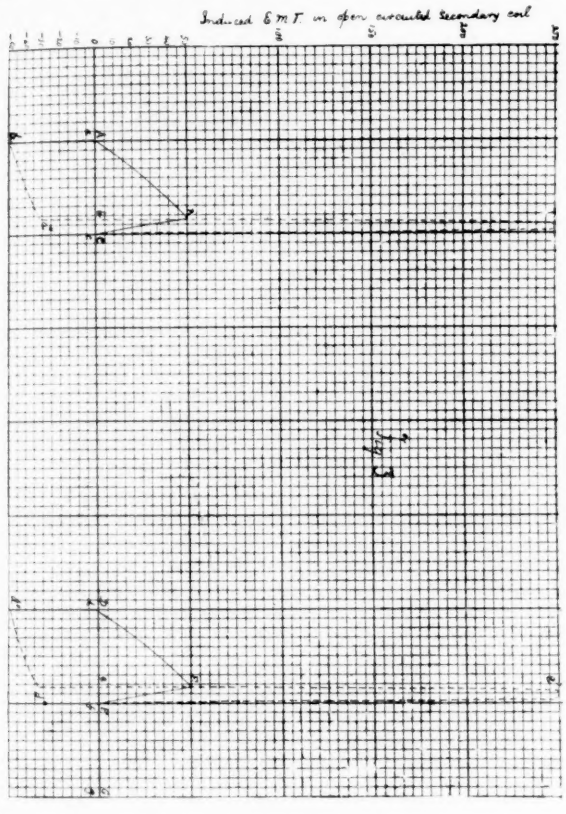


Figure 3.

makes contact between A and B during the sixth part of the cycle, and the current and flux ascend the curved line AB. The spring then leaves the contact and the current and flux fall rapidly to the zero line at C. The dotted line a, a', a" b,c,d, d' d", e,f,g, indicates the corresponding induced electro-motive force in the secondary but open circuited coil. At the moment of contact, a, the induced voltage becomes — 50, and at the moment of breaking contact has fallen to — 35. Immediately the break occurs, the electro-motive force rises quickly to about 250 volts, and after a brief interval falls again to zero. The wave of induced electro-motive force is quite unsymmetrical with respect to the zero line, but the area a, a,' a," b, is equal to the area b, b', c.

Having outlined the electro-magnetic condition of the faradic coil while the secondary coil is disconnected, we may now consider that the secondary circuit is closed through an external resistance.

Currents will now flow through the secondary circuit and circulate round the secondary coil. These currents will generate a new magnetic circuit just as though this coil were excited by those currents as an independent primary. The oscillation of this new flux will induce new electro-motive forces both in the primary and secondary circuits. In the primary the current from the battery will be correspondingly modified during the periods of contact. In the secondary the new electro-motive force known as the counter electro-motive force of self-induction, will always oppose and reduce the main induced electro-motive force, but the balance of the latter will be available for producing currents in the secondary circuit.

For a given external resistance in the secondary circuit, these conditions rapidly arrive at mutual adjustment, such that the primary current at any instant during periods of contact, follows Ohm's law, when the counter electro-motive force of self-induction, and the electro-motive force induced by the secondary currents, are deducted from the electro-motive force of the battery; also the secondary currents at any instant follow Ohm's law, when the counter electro-motive force of self-induction is deducted from the electro-motive force induced by the adjusted primary current.

These conditions, varying with the character of the external secondary circuit, however readily apprehended in the main, are difficult to determine in accurate detail, by either observation or computation. Broadly, however, the effect of loading the secondary circuit of a faradic coil by reducing the external secondary resistance, is to increase the main strength of the primary current supplied, to prolong

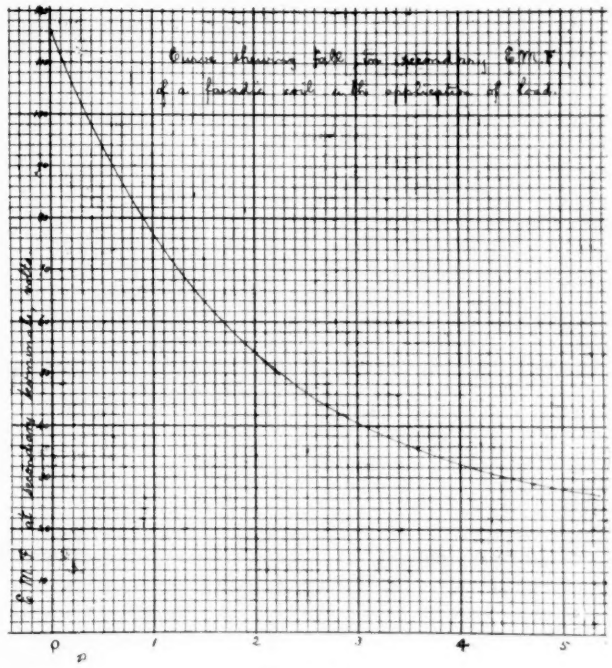


Figure 4.

the duration of the secondary current waves, and to diminish the effective secondary electro-motive force.

Fig. 4, indicates the latter influence as observed in a sample faradic coil whose vibrator made about 150 periods per second.[1] The effective electro-motive force at secondary terminals was 116 volts on open circuit, but when the external resistance was gradually reduced to 5,600 ohms, and the secondary current raised to 5 milliampères, the terminal electro-motive force fell along the curve to 28 volts. That is to say, 28 volts continuously sustained on the 5,600 ohms would generate through that resistance the same amount of heat in a given time as the very variable electro-motive force then active at secondary terminals. The fall of secondary pressure with load was here almost entirely due to the counter electro-motive force of self-induction, aided by the influence of secondary reaction on the primary circuit, for the drop of voltage due to 5 milliampères, the strongest current shown, would only have amounted to 1.4 volts, in the resistance of the secondary coil, a comparatively negligible loss.

[1] This coil had a core 9.25 cms. long and 1 cm. in diameter, formed of iron wires 0.085 cm. in diam. weighing in all 52 gms. The primary coil had 90 feet, 330 turns, 0.76 ohm, and 7.5 millihenrys the secondary coil 1,100 feet, 3,440 turns, 285 ohms, and 456 millihenrys, the mutual induction was 111 millihenrys. The battery had an electro-motive force of 1.5 volts, and a resistance with leads of 0.35 ohm.

The secondary electro-motive force of faradic coils can only be accurately measured by electrometers or electro-static methods, since the feeblest measuring current in the secondary coil exercises so powerful an influence upon apparatus of this size. It is usually more important, however, to know the effective secondary current strength rather than the effective secondary electro-motive force. The duty of a faradic coil is only to supply a certain strength of alternating current, at a given frequency, and of a certain wave character, and the electro-motive force in the coil is merely an attendant property for the production of that current. Any strength and frequency of current within the usual limits can be provided for in design, but probably no two coils give precisely similar wave characters. Short secondary windings with few turns, tend to generate abrupt and sharply varying currents, while long secondary windings with many turns, tend to deliver smoother and more slowly changing current waves, especially at high frequencies and on heavy loads. This is owing to the fact that their large number of turns entails a large linkage with their own flux, and any abrupt change in the currents they deliver sets up immediately a powerful induced opposing electro-motive force checking the variation.

The only advantages that can be gained by modifying the forms of faradic coils operated by existing types of spring vibrator, must be looked for in economy of weight or material, in convenience of operation, or in modification of current wave character. The disadvantage of the vibrator is the irregularity and dissymmetry of the currents it causes to be generated. The ordinary form of spring vibrator rarely supplies more than 250 periods per second, while ribbon vibrators in which the spring is clamped at each end, and capable of being tightened, enable frequencies of 1,000 periods to be readily attained, but the dissymetry and irregularity of current wave character must attach to both forms.

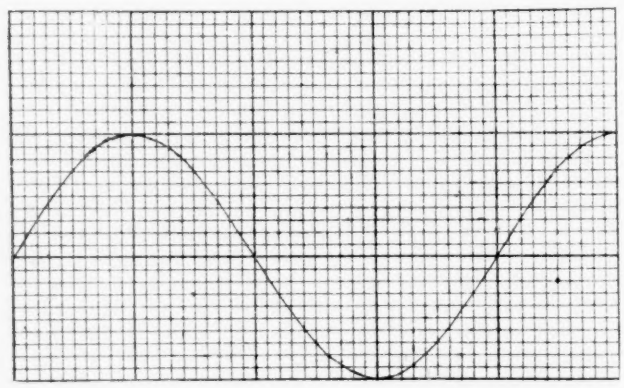


Figure 5.

For measuring the effective current strength in the secondary circuit, a sensitive electro-dynamometer calibrated for milliampères of alternating current is convenient, for the currents are usually feeble, and the resistance and self-induction of the measuring instrument should be reduced as far as possible.

The simplest form of alternating current wave type is the sinusoidal, or simple harmonic wave, represented in Fig. 5. This wave possesses several remarkable properties. It conveys, for example a given quantity of electricity, at a given frequency, with the minimum possible average abruptness or rate of change in current. It is also a persistent form and difficult to destroy. A sinusoidal electro-motive force acting upon the primary of an induction coil, will induce exact or very nearly exact sinusoidal waves of current and induced electro-motive force in the secondary circuit, thus greatly facilitating all measurements, deductions, and reproductions. If an effective current strength of 5 milliampères be supplied by the secondary of an ordinary faradic coil at a frequency of, say, 250 periods there will be considerable uncertainty as to the current wave character and as to the maximum values of current during the cycle, but if the primary coil were excited from a source of sinusoidal currents at the same frequency, all the particulars of the current delivered would be accurately determinable and reproducible. Five milliampères of a sinusoidal alternating current, as measured by a properly calibrated instrument, would always accompany a maximum current in each wave of 7·07 milliampères, but 5 milliampères of current from the ordinary faradic coil might require maxima of 10 milliampères negative, and 70 milliampères positive, in each cycle.

IN MEDICAL INDUCTION COILS, HOW DOES THE CURRENT OF THE PRIMARY DIFFER FROM THAT OF THE SECONDARY? AND WHAT INFLUENCE HAS THIS DIFFERENCE UPON THE RESPECTIVE PHYSIOLOGICAL EFFECTS?

By H. NEWMAN LAWRENCE, Esq. of London, England, was then read by the Secretary in the absence of the author.

I will assume that the currents I am asked to speak about are obtained from an ordinary well made medical coil wherein the primary and secondary helices are quite separate from each other, and the wire of the former is short and of low resistance, while that of the latter is long and of high resistance. Further, that the rate of vibration does not exceed 200 per sec.

Such coils are transformers of electrical energy and not producers of it. They transform a current of low voltage and (comparatively) large ampèrage to one of (comparatively) high voltage and small ampèrage. In this process a portion, often as much as a third or even more, of the initial energy put into the coil is lost in the act of transforming.

The primary current consists of continuous current from the charging source, be it battery or direct current dynamo, interrupted by the vibrator and also possessing the extra current of self induction. The charging current can be measured, both as regards voltage and ampèrage and thus the initial energy put into the coil is arrived at.

The secondary current is a series of alternating impulses derived by induction from the primary, and depending for the form of its resultant energy, i. e., the relative values of voltage and ampèrage, upon the number of turns it makes round the primary, the strength of the charging current and the rate of interruption of the vibrator.

The difference between the two appears to be:

a. As regards direction.

Primary—Flows in one direction, though interrupted at intervals.

Secondary—Flows as a series of impulses, alternating in direction and more or less interrupted owing to the interruptions of the primary. Quite interrupted at low speed vibrations, but wave-like or undulating at high speed.

b. As regards voltage.

Primary—Comparatively low voltage, say 50 volts.

Secondary—Comparatively high voltage, say 300 volts.

c. As regards ampèrage.

Primary—Comparatively great ampèrage say 500 ma.

Secondary—Comparatively small ampèrage say 7 ma.

Perhaps a few figures will help illustrate the transformation of electrical energy in a coil.

Assume that our coil has a maximum of 12 ohms in primary and 1,000 ohms in the secondary.

The initial energy is 500 ma. at 6 volts = 3 watts. This when transformed in the primary by self induction may become 60 ma. at 50 volts or thereabouts.

The secondary, taking the total loss of energy as a third will give out 7 ma. at 300 volts, which equals 2 watts.

Taking these figures as an example it will be seen that the arrangements of the primary current are such that increased ampèrage is obtainable almost up to the full amount in accordance with the possible reduction of a patient's resistance In saying this, however, we must not overlook the fact that the patient is not put into the direct current but into a loop or shunt circuit and consequently only receives such ampèrage as belongs to the shunt in inverse proportion to the resistance in it, i. e., of the patient, electrodes and connections.

In the secondary the resistance of the patient is not of so much importance, as the values of ampèrage and voltage are such that the maximum of the former is decidedly small and the discharge in this respect approximates to that of a static machine.

Physiological effects.

Primary—*Electrolytic* owing to its flow in one direction, but this effect must be small, owing to the interruptions. *Cataphoric* to a like extent and for the same reasons. *Muscle contracting* by reason of the interruptions, these contractions being distinctly painful owing to the sharpness of the interruptions and the strength of ampèrage.

Secondary—Neither *electrolytic* nor *cataphoric*, to any appreciable extent, owing to the alternations and the very small ampèrage. *Muscle contracting* by reason of the alternations, these contractions being less painful than those obtained from the primary, owing to the wave-like

character of the alternations and the very small ampèrage.

My experience is, that the muscle contraction may be produced by an infinitesimal ampèrage, provided it be sufficiently concentrated; and that the pain of such contractions increases with the ampèrage, other things being equal.

Speaking generally, therefore, when muscular contraction is required the primary current is the more painful to use; though at the same time, owing to its definite direction and cataphoric properties it may sometimes be found less painful than the secondary when applied to nerves.

The rate of vibration has a marked influence upon both the primary and the secondary, with their consequent effects; but it does not seem that one is affected by it more than the other, at any rate within the limit named (200 per sec.)

That effects of the electric energy of the secondary, presented as it is with an apparent superfluity of voltage and a corresponding paucity of ampèrage over and above those already referred to may exist, seems possible, but such existence is at present open to doubt.

Probably it is only by investigating the extreme of such condition, as illustrated by Teslaic coils or Dr. Morton's static induction currents, that this problem can be solved. The conditions in each case are similar, i. e., alternations, superfluity of voltage and paucity of ampèrage (these terms are here intended to refer to action upon the body only), and the difference seems to me to be merely one of degree.

In conclusion, I apologize for the sketchy nature of these remarks, but must plead that the subject is a very intricate one, both in its physical and physiological aspects; and that to do the matter justice would require a far abler pen than mine, with time and attention considerably in excess of the limits laid down for this discussion.

(To be continued.)

SCIENTIFIC OBSTETRICS IN PRIVATE PRACTICE.

Read in the Section on Obstetrics and Diseases of Women, at the Forty-fourth Annual Meeting of the American Medical Association.

BY EDWARD P. DAVIS, A. M., M.D.

PROFESSOR OF OBSTETRICS IN THE PHILADELPHIA POLYCLINIC; CLINICAL LECTURER ON OBSTETRICS AND GYNECOLOGY IN JEFFERSON MEDICAL COLLEGE, ETC.

If one were asked the meaning of the word, "scientific," a proper definition would be found in a phrase indicating the possession and dissemination of knowledge. A scientific journal is one that contains and disseminates knowledge of the subject of which it treats. Modern medicine is a science in so far as it contains valuable knowledge concerning human life and disease, and scientific obstetrics is the theory and practice of this branch of medicine, characterized by accurate knowledge.

In this one point of the possession or absence of accurate knowledge rests the difference between the obstetric practice common in the United States twenty-five years ago, and the theory and practice taught at the present day. Impressed with the ability of nature to deal successfully with the emergencies of labor, the practitioner who attended obstetric cases has been wont to comfort himself with the axiom that labor is a natural process, of which he need not seek to ascertain too minutely the details. The condition of pregnancy he regarded as naturally normal, and hence without especial tendency to disease. The lying-in period was to him a natural stage in the life of the individual, and required at his hands no especial scrutiny. An accurate diagnosis as to the factors present in a given labor was rarely made, unless some serious emergency arose.

There have been many reasons for this condition of affairs. The comparatively recent discovery of anesthetics placed at the disposal of obstetricians a means of diagnosis of inestimable value. The employment of antiseptics has rendered possible manipulations for the diagnosis and treatment of labor that formerly were impracticable. Methods of diagnosis are now commonly taught and practiced that, at the period mentioned, were only to be seen in practice in the large hospitals of Europe. Modern obstetrics has, then, great advantages in its opportunities to acquire thorough knowledge of the patient and of her labor, and this, too, without the infliction of additional pain, or the incurment of added risk to life. The characteristic of modern obstetrics is its scientific element, as shown in the accurate knowledge that it requires concerning matters formerly thought to prosper inevitably through the kind coöperation of nature.

Scientific obstetrics is the rule in the modern hospital, but it is often "more honored in the breach than in the observance" in private practice. The reasons for this are many. First, among all, is the common belief, derived largely from physicians and nurses, that, because labor is a natural process it is bound to proceed successfully without intelligent supervision. If this is true, why should a patient tolerate palpation, auscultation, or pelvimetry; or why should she take an anesthetic when the mechanism of labor is interrupted, and a thorough examination of the condition present becomes necessary? A common belief that has descended from the days of the early craniotomist, or man-midwife, as he was termed, is that any interference with labor means, practically, the use of forceps, and hence the objection to critical examination in an emergency. Again, it not infrequently happens that a complicated labor terminates spontaneously, and apparently with success to mother and child. It does not occur to the patient and her friends that complications may have arisen, by reason of the long continuance of the spontaneous labor, that could have been avoided had intelligent interference been practiced.

There is especial need for scientific obstetrics in private practice, because the practitioner is more directly responsible for the condition of his patient than when he is engaged in hospital practice. He lacks in private the experienced observation of trained nurses; he misses the intelligent coöperation of resident physicians; and experiences the great disadvantage that the absence of order and discipline, so common in a private house entails upon him. It is, therefore, especially necessary that he gain thorough knowledge of his patient by personal observation and study, and this knowledge is the essential of scientific obstetrics. The fact that the best results in obstetric practice are observed in hospitals, draws attention to the need for scientific work in private cases.

Omitting a consideration of the minor details of private obstetric practice, we urge the importance of a thorough examination by the practitioner of all pregnant patients. We are well aware of the fact that many cases are not seen by their attendants until labor is far advanced, and no opportunity has been given to examine the patient during pregnancy This is growing to be, however, less the case than formerly, and especially in first pregnancies, when the natural dread of an untried experience brings the patient to the physician comparatively early. This scrutiny of the pregnant patient should be directed especially to ascertaining the condition of the organs of elimination, and the success attending their action. The examination of the urine should be an invariable practice, and valuable information will be given by

ascertaining not only the presence or absence of albumen, but by careful microscopic study of the sediment, and also an estimation of the amount of urea excreted. In the Maternity Department of Jefferson Medical College Hospital we have for some time derived valuable information regarding a patient's eliminative powers by ascertaining the quantity of urea excreted. The presence of albumen is of itself a trifling matter, unless the microscope reveals evidence of decided structural change in the kidney.

At or after the seventh month of pregnancy it becomes the duty of the attendant physician to ascertain the mother's size, and the relative size of the fetus and the position that it occupies in the mother's pelvis. This is a procedure commonly thought unnecessary, and reserved for the lectures of those who teach obstetrics. That such is not the case is well exemplified by two fatal cases, seen by me recently in consultation, in both of which embryotomy upon the dead fetus was necessary. Two well-developed male children were sacrificed to a lack of diagnostic skill on the part of the medical attendant, and to complete the unfortunate history of those two cases, the lives of the mothers were lost on account of the stupid laziness of the physicians, who neglected to use antiseptic precautions during labor. In both cases the child had perished before the consultation, and the embryotomy was performed to spare the medical attendant from reproach and criticism for his failure to deliver the mother. These four lives could have been saved had proper precautions been taken.

The practice of general medicine, including obstetrics, in country towns or villages, often taxes to the utmost the skill and wisdom of the so-called "country doctor." As a result, he has a range of experience unknown to his city brother, and becomes by natural development more assured, self-reliant, and efficient. He is the man who, above all others, can appreciate the value of any method of practice that shall add to the positive knowledge he has of his patient. We do not ask him to provide himself with elaborate or costly pelvimeters, or instruments for diagnosis but, as is our custom in lectures at the Philadelphia Polyclinic, we do urge upon the general practitioner a knowledge and practice of palpation, auscultation, and of such a method or methods of estimating the size of the mother's pelvis and the relative size of the fetus as may be within the scope of his possibilities. No student graduates from a well-appointed medical college who is not in the possession of this knowledge, and certainly the general practitioner already established can not afford to be behind the recent graduate in his resources. The objections urged against the examination of pregnant patients, including the measurement of the pelvis, are based upon needless exposure of the patient, or rude and unskilful manipulation. We have yet to find a private patient, of whatever station in life, who offers the slightest objection when her physician asks that he be given an opportunity to acquire a knowledge that shall the better enable him to care for his patient, particularly if, by so doing, he subjects her to no exposure, nor to tedious and painful manipulation. The abdomen of the patient may remain covered by one thickness of cotton or linen, without in the least inconveniencing the physician, and medical students under my instruction are taught to make their examinations in this manner.

One of the great advantages accompanying this mode of practice is the lessening of vaginal examinations, which are a source of considerable danger to the patient. It is perfectly possible by palpation alone to follow the head of the child as it descends through the pelvic canal and impinges upon the pelvic floor. The character and efficiency of the pains of labor may also thus be accurately observed, and oftentimes, valuable assistance can be rendered by external pressure gently and judiciously applied.

When the mechanism of labor is interrupted, a thorough internal examination is at once indicated. This is often impracticable without the administration of an anesthetic. The practitioner should be prepared to interfere for the assistance of the patient. Forceps should be prepared and ready. The rectum and bladder should be emptied, and, while it is not necessary to state to the patient that an operation is to be performed, yet while she is under the influence of the anesthetic, it may be found wise to terminate the labor. It is often impossible to accurately diagnosticate a faulty position of the head without an anesthetic, the resistance and suffering of the patient being so great that sufficient of the hand can not be introduced to make a thorough examination. When, however, such an examination has been made, and before the examining hand has been withdrawn, the forceps may often be accurately applied to the sides of the child's head, and the labor brought to a successful termination. In other cases, version and extraction may be found necessary. In either event, examination of the patient under anesthesia will spare her hours of suffering, and will often save the life of an infant, and sometimes that of the mother.

The same thoroughness in treatment should obtain in the diagnosis and treatment of injuries to the genital tract. By the term, "laceration of the perineum," we understand a solution of continuity that extends more deeply than the posterior commissure or fourchet. We are aware that a laceration in the median line may extend beyond the fourchet, and still not impair the supports of the uterus to any appreciable degree. When we read and hear of large numbers of cases of labor in which no laceration of the perineum has occurred, we are impressed with the fact that those who record such extraordinary experiences either consider that a laceration of the perineum usually extends into the rectum, or else that they did not accurately examine the cases. The examiner may be readily deceived by a superficial examination by the finger only of the genital canal, soon after labor. Clotted blood often fills a torn surface, giving to the finger the same continuity of smoothness that marks the uninjured mucous membrane. It is only by the thumb and finger, one inserted in the rectum, or, better still, by actual inspection, that the condition of the genital tract after labor can be accurately ascertained. Considerable observation of labor cases, and the careful employment of the best known means for preventing laceration of the perineum, have convinced me beyond reasonable doubt that such an injury is inevitable in many cases. It is negligent not to discover it; it is lazy and dishonest not to endeavor to repair it.

Fortunately, in these matters, public sentiment is demanding the advantages of scientific obstetrics. We recently had occasion to tell a young woman that she had sustained a laceration of moderate extent that ought to be closed. She said that if such was the

case, she earnestly desired that it be at once repaired, as her sister had suffered from the neglect of her physician in not suturing a similar laceration. We are furthermore helped in dealing with these questions by the fact that experience shows us that excellent results may be obtained as late as twenty-four or thirty-six hours after the occurrence of a laceration. It is far better practice to give the patient and physician twenty-four hours' rest after a trying labor, and then, under anesthesia, or with the use of cocain, close a laceration under favorable circumstances, than to further exhaust a weak patient, and with poor light and laboring under great fatigue, to improperly perform the physician's duty. If torn surfaces are found glazed over, the simple expedient of scraping them with the blade of a pair of scissors, or with a curette, will suffice to secure union by first intention.

It is burdening you with a trite observation to speak of the importance of obstetric antisepsis or asepsis, and yet the same reasoning that enables the practitioner to assert that in his 1,000 cases he has never had a lacerated perineum, leads him likewise to claim that in his 1,000 cases he has never had a case of puerperal septic infection. He may have avoided signing a death certificate for a patient recently delivered, the certificate bearing the words, "peritonitis," "blood-poisoning," "inflammation of the bowels," or "puerperal fever"; but cases occasionally perish some weeks after labor, with "jaundice," "pneumonia," "congestion of the liver," or "malaria," that, on closer study will be found to be the results of puerperal septic infection. The faithful practice of aseptic or antiseptic precautions greatly lightens the load of responsibility that the practitioner of obstetrics must carry.

Not the least of the advantages derived from the scientific practice of obstetrics lies in the avoidance of interference by those who have no right to molest the patient. The practitioner's best efforts are often thwarted by ignorant and meddlesome persons. He can not, however, honestly forbid interference, unless he knows positively that there is no occasion to molest the patient. He will obtain the best results in obstetric practice who does the work himself, unless he can command, for certain things, the assistance of a trained nurse. Thoroughness in diagnosis, the faithful observance of antisepsis in treatment, and the protection of his patient from meddlesome interference, will give the best results. Such a method of procedure is thoroughly scientific, for it demands the best knowledge, the most careful and skilful manipulation, and, above all, an honest and fearless attempt to meet the difficulties that may arise. In the present stage of medical science, every woman has a right to demand such treatment, and if a practitioner of medicine can not give his patient such care, he must inevitably, by the inexorable law of the "survival of the fittest," give place to those who can.

PUBLIC HEALTH.

Woman's Health Protective Association.—At the recent Sanitary Convention at Harrisburg, a paper was read by the Director of Public Safety of Philadelphia, Mr. A. M. Beitler, in which he paid a handsome tribute to the recent public work of an organization, the Woman's Health Protective Association of Philadelphia, for the great interest they take in the city's cleanliness and for the success they have had in forwarding complaints to the proper bureau for registry. The Director of Public Safety believes that the women of Philadelphia, through this quite inexpensive organization, will yearly make their work tell on the city's good order and for the observance of sanitary regulations already provided; that they will be potent aids in preserving the health of Philadelphia. This city, by its plan, and its favorable building developments, of a single house to one family, should have the smallest death rate of any in the world, for in no large city are the bulk of the people so comfortably and decently housed. It only needs the education of the individual householder to do his or her part in every way, by vigilance and by voting, by setting an example both in personal care of premises and in reporting at once all nuisances in the neighborhood; an example, also, on the part of each voter in choosing loyal and business men to conduct the city's business in councils; it only needs this to have, in a few years, a transformed city, with clean water to drink, all places of pollution shown up and cleaned out, and not only the smallest death rate in the world, but the most comfortable city to live in. All of this the women may bring about if they will be ceaselessly vigilant and fearlessly outspoken.

Smallpox.—The Reading, Pa., Berks County Board of Health, reports that three new cases of smallpox occurred in that city during the two weeks ending Feb. 5, 1894. These cases occurred prior to Jan. 26, 1894. The total number of cases in that city since the outbreak in February, 1893, have been 713, with 18 deaths to date. Only seven cases are now in the Emergency Hospital, all of whom will shortly be discharged.

An outbreak of smallpox has occurred at Tyrone Forges, near Tyrone, Blair County. The inspector reports seven cases of smallpox and five cases of varioloid as existing on February 8th instant. This epidemic has been traced to Reading, Berks County. One fatal case of smallpox is reported at Puttstown, Bedford County, traced to Tyrone. One case of smallpox exists at Williamsport, Lycoming County, origin not known, and one case of the same disease is reported from Wilkinsburg, near Pittsburg, Allegheny County.

Five cases of smallpox in Pittsburg, Allegheny County, origin unknown. One case of smallpox in Philadelphia, origin unknown. Yours truly, BENJAMIN LEE,
Secretary.

Yellow Fever.—Propositions adopted by the Conference of Gulf Quarantine Authorities, held in New Orleans, Feb. 2 and 3, 1894.

Your committee to whom was referred the consideration of the several propositions relative to the regulations of the fruit trade during the quarantine season, beg to report as follows:

1. In order to secure prompt and reliable information as to the existence of yellow fever in the various ports of the West Indies and Central and South America from which fruit is shipped, it is deemed essential that medical inspectors should be stationed at said ports with the understanding that the inspectors stationed in the larger towns may have jurisdiction over smaller places within the district.

2. The duties of the inspectors shall be to report by each and every vessel, and if necessary by cable when practicable, on the sanitary condition of their respective districts; on the existence of contagious or infectious diseases within the same; on the compliance by vessels with the regulations issued for their guidance and to perform such other duties as may from time to time be required of them.

3. It is recommended that vessels engaged in this trade shall, whenever practicable, be manned by acclimated crews.

4. In the absence of yellow fever, it is considered unnecessary to place any restrictions upon the manner of loading vessels, or upon the character of cargo carried, further than to rigidly exclude household effects, personal baggage, hides, horns, bones and fertilizers.

5. After the discharge of the cargo every vessel shall be subjected to mechanical cleansing by washing, and afterwards thoroughly disinfected.

6. In the event of yellow fever occurring at any fruit port, trade with that port may be continued under the following additional precautions:

Each vessel to be required to carry a duly accredited medical officer whose duty it shall be to see that all quarantine regulations are strictly carried out. No member of the crew shall be allowed to go on shore nor any one, except an acclimated loading crew, to come on board. The loading crew to be kept isolated in a locality sufficiently remote from the infected district and while on board the vessel to be kept separate from the crew. Vessels are not to be allowed to remain at any wharf or near shore after sunset, but must anchor in the open bay at such distance from shore as may be prescribed by the medical officer in charge, and must not return before sunrise. The cargo carried shall be restricted to fruit, rubber and specie. Rubber not to be packed in any textile fabrics, and specie to be disinfected by immersion in boiling water.

7. Should a vessel having yellow fever on board and carrying a cargo of fruit arrive at a quarantine station, the fruit may be lightered by acclimated crews or by the crew of the vessel, after which the vessel shall be subjected to the usual detention and disinfection. The clothing of the laborers to be disinfected by steam heat after the discharge of the cargo shall have been completed.

8. Any vessel engaged in the fruit trade, which carries passengers, should forfeit all special privileges and be subjected to the same quarantine restrictions as other vessels carrying passengers.

9. Inasmuch as the quarantine laws and regulations of the United States as promulgated by the Secretary of the Treasury April 4, 1893, are not in accord with certain of the foregoing propositions, it is recommended that such modifications be made in said laws as shall make them to coincide with the views expressed by this Conference.

G. FARRAR PATTON, M.D.
JEROME COCHRAN, M.D.
R. W. HARGIS, M.D.

SELECTIONS.

Extirpation of the Gall Bladder.—After reviewing the history of this operation which was first attempted by Jean Louis Petit, in 1743, and which was given a renewed impetus by Langenbuschen ten years later, the author then cites two cases operated on by Helferich.

The first was that of a man 21 years of age who first appeared in the clinic Jan. 18, 1892, complaining of a severe hepatic colic to which was added a transient jaundice.

On the under surface of the liver a flat pear-shaped tumor was discovered which proved to be the gall-bladder, its walls thickened and shrunken, and containing nine to ten small faceted stones, after the removal of which the gall-ducts appeared to be intact. Convalescence was retarded by the formation of an abscess, which two weeks later was evacuated through the lower aspect of the abdomen.

The second case, a man 42 years of age, first came under observation in July, 1892. He exhibited a tumor near the under surface of the liver, which during its growth was the cause of much pain and considerable gastric disturbance. The swelling lay in the right hypogastric region, about the size of a man's fist, and disappeared on deep inspiration.

On incision the tumor seemed to consist of the distended gall bladder to which the transverse colon was adherent.

On its lower circumference there was a thimble-shaped pocket or sac, containing about one-fourth liter of a yellow purulent fluid in which were fat corpuscles and sediment. In the lumen of the cystic duct, a gall-stone about the size of a cherry-seed, was so firmly impacted that it could only be removed after first breaking with forceps. As it was so firmly held in its situation by the minute projections on its surface, and as its removal by a lithoclast would injure the duct, it was deemed advisable to extirpate the gall-bladder and duct at a point just above the site of the stone. The remaining portion of the duct was carefully closed with Lembert sutures. Convalescence was uninterrupted, no febrile reaction taking place.

The author then discusses the results of the cholecystectomies; of seventy-two cases observed by himself only sixteen resulted in death, only seven of which could be attributed to the effects of the operation.

The different reports concerning the importance of extirpation of the gall-bladder as an operative procedure, against the operation for gall-stones, have not yet been decided in favor of either.

Lauenstein, however, upon his own experience, is opposed to this operation. He has treated cases exactly like those above related, and quite as successfully, by merely opening the gall-bladder. Among thirty cases in which an operation was indicated, in only two was the gall-bladder removed, and then only on account of a secondary complication in the form of a biliary fistula.—C. LAUENSTEIN in the *Centralblatt für Chirurgie*, January, 1894. [Abstracted for the JOURNAL.]

The Use of Ether in Incarcerated Hernia.—Gussenbaur has tried Finkelstein's method of using ether in incarcerated hernias, on a number of cases from the Prague surgical clinic. Of 135 cases which came under observation, 108 required operative interference, and in only five of these the ether treatment was unsuccessful. The remaining twenty-seven, among which were sixteen inguinal, one ventral and three femoral, were all successful.

Gussenbaur recommends the practice of the American physician, H. Koch, of protecting the thighs and scrotum from the ether fumes. The danger of an explosion when artificial light is used must be borne in mind.

Among the twenty successful cases mentioned above, spontaneous reposition without taxis resulted in three of the cases. One patient succeeded in reducing his hernia himself after the application of the ether. All the others were reduced by the surgeon in from one to three hours. In only two cases was a longer time than three hours required.

Gussenbaur considers this method of ether application worthy of more notice. He believes the results would have been still better had he attempted this operation earlier than he did, as forcible taxis had already been attempted.

It should be understood, however, that the operation of herniotomy should not be entirely excluded. The ether treatment can be tried when this operation is contra-indicated, as in cases where incarceration is accompanied with symptoms of gangrene or peritonitis.—"JAFFE," in the *Centralblatt für Chirurgie*, January, 1894. [Translated for the JOURNAL.]

NECROLOGY.

A. G. Standiford, M.D. of Westville, Ind., died February 10.

J. T. Doyle, M.D. of Wilkesbarre, Pa., died February 10.

Dr. Lingenfelder, 50 years old, of St. Louis, Mo., a passenger on the North German Lloyd steamer, *Spree*, which arrived Feb. 15, from Bremen, died from a disease of the liver on the voyage. His body was sent to St. Louis.

A. Dunlap, M.D. of Springfield, Ohio, died February 17. He was a delegate from the AMERICAN MEDICAL ASSOCIATION to the International Medical Congress in London, Eng., in 1881, and one of the earliest physicians to demonstrate the operation of laparotomy, having performed over 400 operations in various parts of the United States.

THE
Journal of the American Medical Association
PUBLISHED WEEKLY.

SUBSCRIPTION PRICE, INCLUDING POSTAGE:

PER ANNUM, IN ADVANCE $5.00
SINGLE COPIES . . . . . 10 CENTS.

Subscriptions may begin at any time and be sent to

THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION,
No. 68 WABASH AVE., CHICAGO, ILLINOIS.

F. J. REBMAN, LONDON AGENT, 11 Adam Street, STRAND, LONDON.
W. H. LOWDERMILK & CO., WASHINGTON, AGENTS.

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SATURDAY, FEBRUARY 24, 1894.

BRITISH CHOLERA MICROBES.

The press dispatches report that the International Sanitary Commission at Paris, has succeeded in inducing the British Government to agree to proper international quarantine at Red Sea ports, and that the Turkish Government on its part agreed to purify the "holy" well at Mecca.

For this much we should be thankful, but the great measures of sanitation should include the proper enforcement of sanitary regulations of pilgrims to and from the holy city, and beside, some really adequate hygienic measures in India.

The whole world has known for a long time that the awful responsibility of maintaining cholera centers, has rested with the Indian Government and that of Turkey, and that these countries have at last agreed to even this feeble step is acknowledgment that they recognize it.

An international quarantine at Suez has been maintained for several years under French auspices, but the British ships have frequently evaded it, and when proper inspection was submitted to, it was done with an ill grace.

When Great Britain seized the reins of government in India, she became morally responsible for the innumerable cholera centers in her stolen territory. That responsibility can not be shaken off by the Anglican practice of raising a cloud of objections to the theory and practice of quarantine, and the mandate of other nations should be, that England should no longer be allowed to afflict the globe by her indifference to the exportation of cholera microbes.

These microbes once Indian, are now British, and whether or not we agree with MR. HART that they are "waterborne," we can not fail to notice that instead of vigorous action in destroying them, she has heretofore not only placed no obstacle in the way of their exportation, but has actually tried to break down the barriers erected by other nations in self-defense.

SOME OBSERVATIONS ON THE CAUSE OF JAUNDICE.

Jaundice is a condition characterized by a more or less deep yellow, or greenish yellow color of the skin and conjunctiva and by the presence of bile or bile coloring matters of the urine. It invariably depends upon causes which either completely obstruct the flow of bile, or which modify the pressure under which the bile normally flows along the biliary ducts. When, from any cause, the pressure increases in the bile ducts, the bile coloring matter and the bile acids enter the lymphatics of the liver and are conveyed in the lymph through the thoracic duct into the blood.[1]

If the thoracic duct, and as well the common bile duct be ligatured, no jaundice results, and biliary constituents can not be detected in the urine. Dogs survive this operation, sometimes for seventeen days without apparent inconvenience, without jaundice and without bilirubin or bile acids appearing in the urine.

It therefore appears to be proved beyond the possibility of doubt, that it is only through the lymphatic paths that biliary constituents can leave the liver and enter the blood.[2]

The theoretic explanations of "Jaundice Independent of Mechanical Obstruction of the Bile Ducts," given by Dr. Murchison, in Quain's Dictionary of Medicine, and quoted by the recent writers on practice are now obsolete, and it is only within a comparatively recent period that facts have accumulated which prove that jaundice is always due to an obstruction to the normal efflux of already secreted bile. By hepatogenic jaundice is meant jaundice due to obstruction. By hematogenic jaundice is meant non-obstructive jaundice, which does not exist. Stern has shown,[3] that contrary to Harley's views, the bile acids and bile coloring matter are both formed in and by the liver. In ducks and geese, the secretion urine continues after the ligature of all vessels to the liver.

If the liver be extirpated the urine remains free from biliary constituents; Minkoski and Naunyn having discovered that ducks and geese, after the inhalation of arseniuretted hydrogen, excrete bile coloring matter with urine, they exposed them to this agent after removing the liver. In the time intervening between the poisoning, and the death of the bird, urine was excreted which contained hemoglobin, but was free from bile coloring matter.

They thus succeeded in demonstrating that in the

[1] Fleischl, Kunkel, Gamgee, Physiological Chemistry, p. 359.
[2] Gamgee, p. 361.
[3] Gamgee, p. 365.

most typical cases of supposed hemotogenic jaundice, the excretion of bilirubin is necessarily connected with and dependent on, the liver.[4]

More recent research has proved that there is not, at present, any sound basis for the conclusion that the bile pigment occurring in jaundice has any source other than the liver. According to our view, every form of jaundice is occasioned by obstruction.[5]

THE DIFFUSION OF SMALLPOX.

In Pennsylvania there are not as many cases of smallpox as there were on January 1, and although the disease seems to be under control there were three fresh cases at Reading within the last two weeks, one at Saxton, Bedford County, one death at Pittstown, Bedford County, and seven cases of smallpox and five of varioloid at Tyrone Forges, the disease at the latter locality traced directly to Reading. A mild case of varioloid has also been found in Philadelphia in the person of a young lady who contracted the disease in Connecticut. This is the first case in that city for some time.

In New York City the number of cases is gradually increasing, and there are cases at several points throughout the State. There are still cases at Winchester and Norfolk, Conn., and on February 9 a case was found on a railroad train in that State. The disease still exists in Boston, but seems to have been stamped out at Somerville, Worcester, Holyoke, Lowell and Brookline.

Fresh cases are reported at Lewisburg, West Va., and a case of varioloid at Walliston, Anson County, N. C., one at Shelby, Ohio, one at South Bend, Ind., also at Otsego, Mich., and at Janesville and Milwaukee, Wis. Cases are also reported at Manhattan, Round Grove, and the poorhouse, Edwardsville, Ill., Duluth, Minn., and Luverne, Keosauqua, Marion, Council Bluffs, Keokuk, Fort Madison and New Hampton, Iowa. The Chicago health authorities daily find new cases.

Taking everything into consideration, the efforts to control and prevent the spread of the disease have been very successful, especially in some localities, where the infection did not spread beyond the first cases attacked. The Wisconsin and Iowa State Boards of Health have ordered the vaccination of all the school children in their respective States, and the Illinois Board has extended the scope of the vaccination order of that Board. It is undoubtedly the duty of the health authorities to secure the vaccinal protection of every one they possibly can. We shall no doubt have many more outbreaks, owing to the diffusion of the contagion over so large an area, in addition to the fact of the presence of the disease in New York and Chicago, the greatest distributing centers in the country, and the increase of the tramp element. The suggestion of DR. PROBST, Secretary of the Ohio State Board of Health, that all railway employes should be vaccinated, is a good one, and should be carried out. The low death rate by the disease is still notable, and another feature that is marked is the great number of cases that are found in general hospitals in the large cities. The latter is, no doubt, owing to the fact that the poor seek hospital shelter much sooner than usual, and thus make the control of this disease more difficult.

There are now in the United States twenty-eight suspected places, a greater number than we have had at any time since 1882. In England there are about the same number of places, but we do not have so many cases. There are now in the United States about 375 cases, while the fresh cases for the past week have been about 175.

In Paris, smallpox has been prevailing for some time and is increasing, owing to the neglect of vaccination. Vaccination matinees have been the order there, but this has not been found sufficient to check the disease, and there is now to be general and obligatory vaccination in all the communal schools, primary and secondary, and the Assistance Publique has organized a system of public gratuitous vaccination, at stated hours, at twelve of the hospitals located in different parts of the city.

THE CAUSE OF DEATH.

The cause of death and a number of other medico-legal questions, as many as would hardly, ordinarily, be raised and decided in a half dozen or more cases, are exhaustively considered by the United States Circuit Court of Appeals, Sixth Circuit, in the case of the Manufacturers' Accident Indemnity Company v. Dorgan, decided Nov. 6, 1893, and just reported in the advance sheets of 58 Federal Reporter, page 945. This was an action brought to recover for the death of a person who had gone on a fishing excursion, and was found by one of his companions, twenty minutes after he was seen playing a trout, lying in the brook, with his face downward and submerged in six inches of water, dead. The points decided of special interest to the medical profession, taken seriatim, are as follows:

While the opinion of a non-expert witness is not admissible in evidence on facts which it is possible for him to detail to the jury so that the latter can draw its own inference therefrom, a physician who performs an autopsy may be asked his judgment of the conditions which he has found in the body of the deceased person, and what they indicate as to the cause of death.

But a question is incompetent, which asks a physician, not present at an autopsy, whether in his judgment, from the testimony offered with regard to the autopsy, he would say that the autopsy was such as

[4] Gamgee, p. 365.
[5] Bungee Physiological Chemistry, p. 379-380.

to enable a physician to state with any degree of certainty the cause of death, because it asks him to make his own inference as to what the evidence of the other witnesses tends to show, and then, upon such inference to give his opinion. To properly elicit his opinion as to the character of the autopsy, and its usefulness in showing the cause of death, counsel should state the scope and character of the autopsy, as he understands it, so that the jury, in weighing the answer of the witness, can know exactly upon what facts it is based. The difference between this question and a similar one put to the physician performing the autopsy, is that here the witness is asked to weigh other men's evidence, a function peculiarly belonging to the jury, while there the witness is asked an expert opinion of bodily conditions seen with his own eyes. Had the physician, whose judgment of the autopsy is thus asked, been present at the autopsy, a question calling for his opinion as to its evidential weight in determining the cause of death would be a proper one.

A physician who has made an autopsy may testify as to whether there was any occasion for making other than those he made, as, for example, an air or water test with the heart of the deceased, where the sufficiency of the autopsy to show the normal or abnormal condition of the heart is questioned because of a failure to make such tests. He may also be asked as to the purpose and scope of his investigation. And on the same ground, his testimony that he cut open and examined the stomach to see if there was any trace of alcohol in it may be corroborated by showing that his attention had been directed to the fact that deceased had been drinking on the day of his death.

The words, "voluntary exposure, unnecessary danger, and hazardous adventure," as used in an insurance policy exempting the insurer from liability for death produced by such exposure, do not refer to such exposure as men usually are going to take—such as is incident to the ordinary habits and customs of life. In order to constitute a defense as a contributing cause, the exposure must be something beyond the ordinary, or a wanton, a piece of gross carelessness, as we would term such in our designation of a person's conduct in the usual walks of life.

An involuntary death by drowning is a death by external, violent, and accidental means.

If a person suffers death by drowning, no matter what is the cause of his falling into the water, whether disease or a slipping, the drowning, in such case, will be the proximate and sole cause of the disability or death, unless it appears that death would have been the result, even had there been no water at hand to fall into. The disease is but the condition; the drowning is the moving, sole, and proximate cause. A precedent on this point is furnished in a case where the insured was seized with an epileptic fit and fell into a stream, and was there drowned while suffering from the fit. It was there held that the death was within the risk covered by the policy, and not excluded by a provision of the policy that it should not extend to any injury caused by, or arising from, natural disease or weakness, or exhaustion consequent upon disease. This doctrine has also been approved by the Supreme Court of the United States.

On the other hand, an "accidental" death by drowning results from, and is caused indirectly by, fits, vertigo, or other disease, if the fall into the water, from which drowning ensues, is caused by such disease. And if an insurance policy provided that it should not apply to an accident to which a fit contributed indirectly, the insurer would not, under some of the decisions be liable.

The court concludes that the jury in this case might, from the circumstances, properly have found the verdict which they did find, namely, that the unconscious and helpless condition of the insured in which drowning ensued, arose, not from disease, but from indigestion or want of food, or some other temporary cause, and that judgment of the court below, on the verdict in favor of the beneficiary of the insurance policy in controversy should be affirmed.

CORRESPONDENCE.

Medical Legislation in Ohio.

TROY, OHIO. February, 1894.

To the Editor:—I desire to call the attention of the members of the AMERICAN MEDICAL ASSOCIATION in Ohio, to a medical bill now pending in the Legislature, known as the Mosgrove substitute bill. This bill provides for an examining and registry board composed of seven physicians as follows: "Representation shall be given to the different schools of practice in the State as nearly as possible in proportion to their *numerical strength in the State*, provided, however, that *no one school of practice shall have a majority of the whole board*." The last clause of the bill destroys the apparent fairness of the first one, inasmuch as there are now engaged in active practice in the State six regular physicians to one of all other schools combined. The composition of this board under the law would necessarily be four irregulars and three regulars, the one irregular in active practice in the State would be represented by four members of the board, while the six regulars in the State would be represented by three on the board, a shameful discrimination against the majority and against the regular school. The irregulars having the majority would be able to control the board and elect a president, secretary and treasurer of their own liking; have power to establish grades determining the standing of medical colleges; countersign permits to practice and in fact practically control the business affairs of the board. The irregulars can always be relied on to stand together in a question against the regular profession and as four is a majority over three, we would be outvoted every time when a question of schools was involved.

The Code of Ethics of the AMERICAN MEDICAL ASSOCIATION, Art. IV, Sec. 1, forbids the holding of professional consultation with irregulars. Yet this bill puts these unworthy